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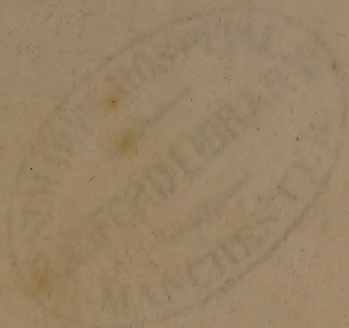












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ESSAY  
ON  
RESPIRATION.

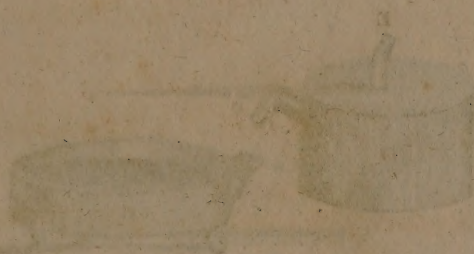
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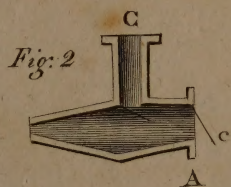




RESPIRATION

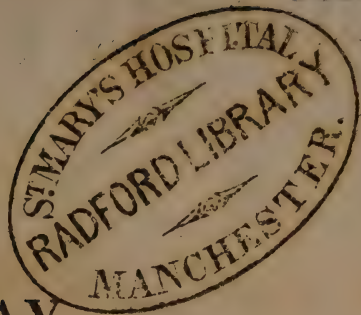


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AN

ESSAY

ON

RESPIRATION.

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PARTS I. AND II.

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By JOHN BOSTOCK, M. D.

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LIVERPOOL:

PRINTED BY J. M<sup>c</sup>CREERY,  
FOR MESSRS. LONGMAN AND REES, LONDON.

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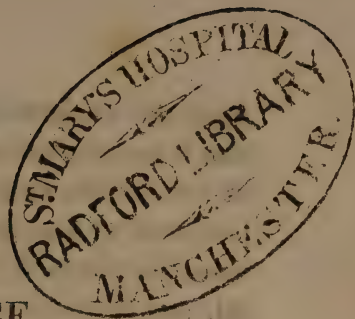
1804.



TO  
THE REV. JOHN YATES,  
AS A TESTIMONY  
OF RESPECT, GRATITUDE, AND  
AFFECTION,  
THIS ESSAY IS INSCRIBED,  
BY  
THE AUTHOR







## PREFACE.

DURING the course of my attendance upon the fever-wards of this town, I had been frequently led to notice the rapid changes of temperature which the body experiences in this disease, and I felt desirous to ascertain how far they could be reconciled to the modern doctrines of animal heat. For this purpose, I entered upon a course of experiments, respecting the chemical state of the respiration in fever, and I afterwards determined to extend my inquiries to other states of the body, either natural or morbid, in which it might be supposed that this function would be affected. As however I was aware that the subject was not altogether new, it was proper to begin by making myself acquainted with what had been previously done by others, not only on this particular topic, but respecting respiration in general. It is well known, that this part of physiology has derived the most important assistance from

from the modern discoveries in chemistry, and that it has also immediately engaged the attention of some of the most distinguished among the philosophers of the present period. Their labours have, for the most part, been peculiarly successful, and by their means we have obtained an insight into some parts of the animal œconomy, which were before concealed in impenetrable obscurity. But the information that has been acquired, still remains dispersed through a great variety of publications, some of which are voluminous, and others not easily to be procured; and in consequence of the rapid advances of chemical knowledge, it happens in a few instances, that the conclusions even of the most judicious physiologists are contradicted by subsequent experiments. I conceived therefore, that my first object would be to collect the best authenticated facts, and the most valuable opinions that had been advanced, and to arrange them in such a manner, as to present a correct idea of the present state of our knowledge.

The only connected view of the modern doctrines of respiration, which we have in our language, is contained in an article of the Supplement to the Encyclopædia Britannica, written by Dr. Thomson; and a section in the system of chemistry



chemistry published by the same gentleman. I have demonstrated my idea of the ability of this author by the frequent reference which I have made to his writings; but the nature of the works restricted him within very confined limits. It was not without a degree of surprise and regret, that I perused the account of respiration, in the system of physiology which is compiled for the use of the first medical school now in existence, a new edition of which was published in the year 1791, under the immediate direction of a gentleman of talents and information, and under the patronage of a University, which ranked among its professors the names of Monro, Black, and Rutherford. The lectures on the animal œconomy, which were delivered at Edinburgh by Mr. Allen, were indeed of such a nature as to supply every deficiency; it is much to be lamented that they are now discontinued, and still more, that there is no prospect of their publication; had that been the case, the following pages would have been entirely superceded.

Every one who is acquainted with the nature of physiological experiments upon the living body, may easily imagine, that as I advanced in my proposed plan, many unforeseen difficulties occurred, and many new subjects of inquiry presented

sented themselves, so that the probable period at  
 which I may expect to have them in a state fitted  
 to meet the public eye, seems scarcely less re-  
 mote than at their commencement. Under these  
 circumstances, I have been induced to present to  
 the public this essay in its present state; should  
 any unforeseen accident prevent the completion  
 of my plan, I have some expectation, that this  
 portion of it will not be without its use; it may  
 at least serve to remove some obstacles from the  
 path of the future inquirer, and may have an  
 effect in directing the attention to one of the  
 most interesting departments of natural know-  
 ledge.

*Liverpool,*  
*Feb. 24th, 1804.*

## INTRODUCTION.

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**ALL** animals, however different in their structure and functions, appear to agree in possessing organs appropriated to the purpose of respiration (*a*) In man, quadrupeds, birds, and the amphibia, they consist of a cavity, with the necessary appendages, which alternately receives and emits a portion of the air of the atmosphere. Fish, in consequence of the medium in which they are immersed, perform this function by a different apparatus; they are furnished with a passage communicating with the fauces or œsophagus, and terminating in the external surface of the body, through which a part of the water received into the mouth is forcibly propelled. In this passage, the branchiæ, or gills, are situated, and the blood which circulates in their fringed extre-

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mities,

(*a*) Note 1.

mities, is thus exposed to the action of a quantity of air, which the water always holds in solution. In many of the insects and less perfect animals, the respiratory organs consist merely of a number of tubes or pores, provided with open mouths, and which simply admit the external air to be received into them. (a) It appears, however, that notwithstanding the difference in the structure and mechanism of these parts, they are all conducive to the same operation, and answer the same general purposes in the animal œconomy. (b) In treating upon this subject, I shall begin by giving an account of the process of respiration; I shall in the second part point out its direct effects; in the third part, the different affections of respiration will be noticed, whether occasioned by the various natural situations in which the body is placed, or by the effects of morbid causes operating upon the system; I shall conclude by investigating its uses, and by an attempt to ascertain the connexion which subsists between respiration and the other functions.

(a) Note 2.

(b) Note 3.

## PART I.

# AN ACCOUNT OF THE PROCESS OF RESPIRATION.

## CHAP. I

*A Description of the Human Organs of Respiration.*

THE human organs of respiration are the lungs, the trachea, with its various ramifications, the pulmonary system of sanguiferous vessels, and the diaphragm. The two first of these are the apparatus by which the external air is conveyed and received into the appropriate cells. The pulmonary arteries and veins are employed in carrying the blood through the lungs in such a manner, that it may undergo the necessary change from the action of the air, while the last is the principal agent in the alternate enlargement and contraction of the cavity of the thorax.

The trachea, is a long tube, composed of cartilaginous rings, connected together by elastic ligaments.



ligaments; it commences in the fauces, and descends into the thorax, where it divides into two branches, which communicate with the two divisions of the lungs. Each of these is subdivided into smaller branches, called the bronchia, which again ramify into others still more minute, until at length they terminate in what are called the vesicles, or air cells of the lungs. In man, and the warm-blooded quadrupeds, the cartilaginous rings are incomplete at the back part, where they lie contiguous to the œsophagus; this deficiency is filled up by cellular substance, furnished with muscular fibres. These fibres are placed both longitudinally and transversely, so that by their contraction, the trachea is diminished, as well in its length as in its diameter. The action of the longitudinal fibres is also assisted by others, which pass across the intervals between each successive ring. As the bronchia decrease in size, their cartilaginous rings become less and less apparent, and at length, when they terminate in the vesicles, their structure is altogether membranous. A number of the air cells, with their accompanying vessels, are connected together by cellular substance, and form what are called lobules; these are again united into larger lobules, and these into divisions of more considerable

derable size, which constitute the lobes of the lungs.(a)

The pulmonary system of sanguiferous vessels, forms the most important part of the respiratory organs, and that to which all the rest may be considered as subservient. After the blood has completed its circulation through the body, it is returned by the vena cava to the right side of the heart. From this cavity, it is propelled through the lungs, along the branches of the pulmonary artery, which ramify with almost inconceivable minuteness over the bronchial air cells, forming what is called the rete mirabile of Malpighi; it is then collected by the pulmonary veins, and returned to the left auricle of the heart.

The pulmonary arteries and veins, with the air vessels, and the accompanying lymphatics and nerves, are all connected together by cellular substance, and compose the lungs, two spongy masses which completely fill the thorax; the internal surface of this cavity is lined with a membrane, called the pleura, which is reflected over the surface of the lungs; it is ascertained, that in all the different states of respiration, the two parts of the pleura

pleura are in perfect contact. (a) The diaphragm is a strong, expanded, and peculiarly irritable muscle, which separates the contents of the two great cavities of the body; in its natural state, it assumes an arched form, convex with respect to the thorax, but when in a state of contraction, its curvature is diminished, and consequently the cavity of the thorax becomes increased.

(a) Note 5.

CHAP.

## CHAP. II.

*A Description of the Mechanism of Respiration.*

**RESPIRATION** consists in the reception and emission of air into, and out of the bronchial vessels of the lungs, by means of the alternate enlargement and contraction of the cavity of the thorax. When the body is in its natural position, and the muscles are relaxed, the lungs are in a state of expiration; but after they have continued in this condition for only a few seconds, we are under the necessity of inhaling a quantity of fresh atmospheric air. For this purpose the diaphragm is contracted, and thus rendered less convex towards the thorax, by which means the capacity of the chest is augmented. The intercostal muscles are, at the same time, thrown into action; and by elevating the arch of the ribs, and thus increasing the distance between the sternum and the spine, still farther tend to enlarge the dimensions of the thorax. The diaphragm is, however, the chief agent

agent in producing this effect; *(a)* and anatomists are generally agreed in supposing, that the use of the intercostals consists principally in fixing the lower ribs, so as to prevent their being drawn down by the action of the diaphragm, and thus obviating the effects of its contraction. *(b)* The contraction of this muscle in some degree displaces the viscera which lie contiguous to it, and pushing them against the parietes of the abdomen, causes a protrusion of the straight and oblique muscles which compose its anterior boundary. The capacity of the thorax being augmented, and a free passage being left by the trachea for the admission of the air, a portion of it will necessarily rush into the lungs, in order to maintain an equilibrium between the external and internal pressure. *(c)* The contraction of the diaphragm and intercostals soon ceases, the elasticity of the cartilages of the ribs re-acts, and the parts return to their natural state; at the same time a degree of contraction takes place in the muscles of the abdomen, and the viscera are restored to their former position; the thorax is thus reduced to its original capacity, and a portion of air is consequently expelled from the lungs. *(d)*

We

*(a)* Note 6.

*(b)* Note 7.

*(c)* Note 8.

*(d)* Note 9.



We have now been describing what takes place in an act of ordinary inspiration ; but this function, though so essential to life that it cannot be interrupted for the shortest interval, without imminent danger, is, at the same time, so far under the control of the will, as that, according to inclination or necessity, it may be exercised in very different degrees. When we are desirous of making a full inspiration, besides the diaphragm and intercostals, we call into action the external muscles of the breast, those of the shoulder and other neighbouring parts, which, by elevating the ribs and sternum, still farther increase the capacity of the thorax.(a) When, on the contrary, we wish to produce a full expiration, the abdominal muscles are more strongly contracted, the viscera are thus pushed more forcibly against the diaphragm, and its convexity towards the thorax is increased.

The grand object of respiration is to effect a change in the blood, by the operation of the atmospheric air ; in order to accomplish this end, it is necessary, both that a fresh supply of air be continually offered to the blood, and also that as large a surface of blood as possible be presented to the air.

(a) Note 10.

air. The fabric and mechanism of the lungs are adapted to produce these effects in the most complete degree : the very act of respiration consists in perpetually changing the air of the lungs, and the manner in which the blood of the pulmonary artery is distributed over the surface of the vesicles, in branches of almost inconceivable minuteness, affords the best opportunity to each particle of blood for undergoing the action of the air. Several physiologists, and among others Dr. Hales and Dr. Keill, (a) have attempted to estimate the number of the bronchial vesicles, and the extent of the membrane lining them. The results differ so much as to demonstrate the fallacy of their calculations ; but they agree in conceiving the membrane to be many times more extensive than the surface of the whole body.

(a) *Keill, Tent. Med. Phys. p. 80. Hales's Stat. Essays. v. i. p. 241.*

## CHAP. III.



*Inquiry into the Bulk of a single Inspiration,  
and into the Capacity of the Thorax in its dif-  
ferent States of Distension.*

AFTER having observed the manner in which the act of respiration is effected, it naturally becomes an object of attention to determine the capacity of the lungs, and the quantity of air received and emitted by them. There are several circumstances which render the latter part of this inquiry of peculiarly difficult research, and notwithstanding the labors of several skilful experimenters, the object of their investigation has not been satisfactorily ascertained. Owing to a difference in stature, and in the peculiar conformation of the thorax, probably also in the constitution and habits of individuals, great varieties prevail in the quantities of air respired by different persons; and in the same person it is well known, that the respiration is materially influenced by the degree  
of

of muscular exertion, the state of the stomach, the mental impressions, and the powers of volition; all, therefore, that can be effected by experiment, is to ascertain the average quantity of air respired in that state of the body, when it is least under the influence of external agents.

Many calculations were made by the older writers, to determine the capacity of the lungs, and the change which it experiences by respiration, but as their estimates were founded rather upon hypothetical reasoning than upon experiment, they were for the most part remote from the truth. Borelli is the earliest physiologist who established an experimental inquiry into the quantity of air received by a single inspiration. (a) Dr. Jurin afterwards improved upon this method, and obtained results which appear to have been tolerably accurate. (b) His experiments consisted simply in receiving the air which was expelled from the lungs into a bladder, and by repeating this process for a sufficient number of times, he concluded, that the quantity of air employed in a single act of respiration amounts to 40 cubic inches. No additional information seems to have been

(a) *De motu Anim.* p. 2. prop. 81.

(b) *Halleri El. Phys. lib. viii. sect. 3. 8. Hales's Essays* v. i. p. 243.



been acquired from the time of Jurin until the publication of Dr. Goodwyn's Essay on the Connexion of Life with Respiration. (a) This author enters fully into the discussion of the questions respecting the capacity of the thorax, and the quantity of air received in a single inspiration, and he instituted a number of ingenious and elaborate experiments for the purpose of ascertaining these points.

His work commences with an attempt to measure the quantity of air left in the lungs after a complete expiration. Every animal, he remarks, immediately before death, produces a full expiration, and therefore, by ascertaining the capacity of the thorax in the dead subject, we obtain a solution of the proposed inquiry. The diaphragm being the only part of the chest which remains moveable, he endeavoured to fix it by applying a firm compress about the upper part of the abdomen. An opening was then made into the thorax, and the lungs, collapsing by their natural elasticity, expelled the air which they previously contained, and thus left a cavity between the pleuræ, which he filled with water. This water he conceived would exactly measure the space previously occupied

(a) Note 11.



occupied by the air contained in the lungs, and from the average of four different experiments, he concluded the quantity of air left in the lungs after a complete expiration, to be 109 cubic inches.(a)

Dr. Goodwyn next attempted to ascertain the quantity of air received into the lungs by a single inspiration, and for this purpose he used the following apparatus.(b) D is a vessel with two openings, through one of which E he inspired, while the other c was immersed in water contained in the dish G; a quantity of water equal to the volume of air inspired will necessarily pass into the vessel D, and from the weight of this water, it is easy to estimate the bulk of the air taken into the lungs. By taking the average of thirty inspirations, and by using the necessary precautions, the quantity appeared to be twelve cubic inches, which when it entered the lungs, he supposed would, by their higher temperature, be expanded to fourteen cubic inches: fourteen cubic inches therefore he estimates to be the quantity of air employed in an ordinary act of respiration. Having previously found the quantity

(a) Note 12. *Connexion of Life with Respiration*, p. 26.

(b) Vide Fig. 1.

tity of air left in the lungs after expiration to be 109 inches, he concludes, that the difference of capacity which the lungs experience in the alternations of respiration, is in the proportion of 109 to  $(109 + 14)$  123. (a) These numbers are much less than the quantities assigned by preceding writers, particularly by Dr. Jurin; it will therefore be proper to examine how far the experiments and observations of Dr. Goodwyn may be found objectionable.

The position, that every animal makes a complete expiration before death, which forms the basis of his experiments, is in the main true; yet there is every reason to suppose, that it will require many restrictions before it can be admitted as the first step in a physiological calculation. According to the nature of the disease or accident producing death, the state of the lungs must vary considerably. In many instances, the respiration is directly impeded by an obstacle preventing the ingress and egress of the atmospherical air out of the bronchia; in some cases, the air when admitted, is prevented from undergoing its appropriate change from the action of the blood, while in a large class of diseases, life is extinguished by

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the gradual cessation of the powers, which enable the heart to propel the blood through the pulmonary vessels. To determine precisely, what is the different condition of the lungs in these different circumstances, would require a long and difficult investigation ; it is sufficient for the present purpose to state these differences, as it is evident that they must produce different effects upon the organs of respiration.

The method which Dr. Goodwyn employed to retain the form of the thorax, lies open to some objections. With whatever care the compress be applied about the region of the diaphragm, there is still reason to believe, that a sufficient power might push it down upon the viscera contiguous to it, and so cause them to protrude into the loins, the groin, or some other part of the parietes of the abdomen, to which the bandages were not applied. Mr. Coleman farther urges against Dr. Goodwyn's experiments, that after a complete expiration, the diaphragm is raised as high as the 4th or 5th rib, but that a compress about the upper part of the abdomen, would be incapable of retaining it in this position, and that the ribs themselves will also descend by the pressure of the water admitted into the cavity of the  
thorax,

thorax, and will consequently draw down the whole substance of the diaphragm. (a)

Dr. Goodwyn appears also to have been mistaken in conceiving, that a complete collapse of the lungs is produced by the admission of water into the cavity of the thorax. (b) A degree of pressure will in this case be exercised upon the surface of the lungs, which will produce a material contraction in their bulk, but it will not obliterate the cavities of the bronchial vesicles, an effect which must take place before all the contained air can be expelled. Both from the shape and texture of these organs, and from actual experiment, we know that scarcely any degree of external force can so far evacuate the contents of the lungs, as to render them specifically heavier than water. (c) Upon the whole we may conclude that Dr. Goodwyn's estimate of the capacity of the lungs after a complete expiration is not unobjectionable, yet the experiments must be considered as valuable, and as affording results probably not very remote from the truth.

The author will be found to be less fortunate

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(a) *Coleman on Resp.* p. 89. (b) *Goodwyn*, p. 24.

(c) *Boer. prælect.* t. v. p. 2, p. 438. *Petit, Mem. Acad.* 1733.

in his second subject of inquiry, viz. respecting the quantity of air received by a single inspiration. He remarks himself, that in breathing through the tube E, the water is raised out of the vessel G, contrary to the power of gravity, and that a greater effort is therefore necessary to receive the due quantity of air into the lungs. (a) He consequently attempted to obviate this source of inaccuracy, but the nature of the apparatus seems to render this impracticable. Dr. Menzies has also pointed out another cause of error in this experiment; when the mouth is removed from the tube, the external air will immediately rush in and repel part of the water into the vessel G, and consequently the measure of an inspiration will be estimated below the truth; that this actually takes place Dr. Menzies found by adding valves to the apparatus. (b)

A more important error into which Dr. Goodwyn has fallen, consists in the comparative estimate which he forms of the capacity of the lungs in the ordinary states of inspiration and expiration. He calculates the quantity of air left in them after a complete expiration to be 109 cubic inches, and he supposes that 14 inches are received

(a) Goodwyn, p. 33, 4. (b) Menzies on Respiration, p. 19.



received at each inspiration ; hence he infers, that in the ordinary process of respiration, the lungs experience a dilatation and contraction from 123 to 109 cubic inches. (a) But it is obvious that the author here confounds together an ordinary and a complete expiration.

The quantity of air emitted from the lungs is, within certain limits, under the control of the will, so that we are enabled by a voluntary effort to expel a considerable quantity of air after an ordinary expiration. The amount of this quantity has been variously estimated, from 220 to 70 cubic inches ; we shall probably be not far from the truth in fixing it at 170 cubic inches. (b) Yet it is certain, that after the most powerful voluntary effort of expiration, the lungs will not be completely emptied, they will then be only reduced to the state in which Dr. Goodwyn made his experiments, when he found them to contain 109 cubic inches.

Admitting therefore the justice of his calculations, the inference which he draws from them will be inadmissible ; the difference of capacity in the states of expiration and of inspiration will

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be,

(a) *Goodwyn, p. 36, 7.* (b) *Note 13.*

be, the quantity of air that can be expelled after an ordinary expiration, (which I have estimated at 170 cubic inches) added to 109, the quantity still remaining in the lungs, in all 279 cubic inches ; and 279 added to 14, the measure of a single inspiration, equal to 293 inches ; but we shall find that this measure of a single inspiration is not correct. From this view of Dr. Goodwyn's work it appears, that though his experiments were ingeniously contrived, and appear to have been carefully executed, yet that some of his most important conclusions are erroneous.

A short time after the appearance of Dr. Goodwyn's work, Dr. Menzies published his *Essay on Respiration*. He begins by instituting an inquiry into the quantity of air employed in a single act of respiration, and also into the capacity of the thorax, as Dr. Goodwyn had previously done ; but he arrives at conclusions which are materially different, and which there is every reason to consider as much more nearly approaching the truth. He procured an allantoid capable of containing 2400 cubic inches, which was fixed to a tube furnished with two valves. (a) The allantoid was placed at the valve c, so as to receive

(a) Vide Fig. 2.

ceive only the air of expiration, and he continued breathing into it until it was completely filled. By comparing the number of expirations with the bulk of the allantoid, he calculated that each consisted of somewhat more than 40 cubic inches. (a) This result he confirmed by fixing another allantoid filled with air to the tube C, and from observing the number of inspirations which this second allantoid afforded, he found the bulk of each inspiration to correspond nearly with his previous estimate of a single expiration. Not satisfied with these calculations, Dr. Menzies had recourse to experiments of a different kind. A man was placed in a large vessel of water, warmed nearly to the temperature of the blood, and the degree of ascent or descent of the water in the upper part of the vessel being accurately noted, from these data it was easy to form an estimate of the alteration in size which the thorax undergoes during respiration. (b) The trials made with this apparatus corresponded remarkably with those performed by means of the allantoid, and thus afforded a striking proof of their correctness. In the experiments of Dr. Menzies there appears indeed no assignable cause of error, and their coincidence with each other, and also with those of

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(a) *Menzies*, p. 30. (b) p. 24—30.

Jurin mentioned above, must be considered as a powerful argument in favor of their accuracy.

In attempting to estimate the capacity of the lungs, Dr. Menzies adopts Dr. Goodwyn's estimate of 109 cubic inches, as the measure of the air left in them after a complete expiration, and adding to this quantity 70 cubic inches, which he found he could still expel after an ordinary expiration, he supposes 179 inches to be the bulk of the thorax in the state of ordinary expiration; to this he adds 40 cubic inches, which gives 219 as the capacity of the lungs after an ordinary inspiration. (a) These calculations of the bulk of the thorax are probably below the truth, as there is no doubt that considerably more than 70 inches can be expelled from the lungs by employing a powerful effort of expiration.

Mr. Davy in his researches concerning the nitrous oxide, performed some experiments for the purpose of ascertaining the capacity of the lungs in their different states of distension, and the quantity of air received into them by a single inspiration. In order to determine this latter point, he merely practised respiration in a peculiar  
pneumatic

(a) Menzies p. 32.

pneumatic apparatus, until he had acquired the power of breathing in it without any extraordinary labour or attention, in a perfectly natural manner. From a number of trials he conceived, that at each inspiration he took into the lungs no more than 13 cubic inches. (a) He also attempted to discover the capacity of his thorax in a state of complete expiration; he proceeded to the solution of this problem in the following manner. He endeavoured to prove by previous experiment, that when a quantity of hydrogene is respired it is neither absorbed by the blood nor in any way altered, but is simply mixed with the air contained in the lungs. He accordingly inspired a quantity of this gas, and then making a complete expiration, he ascertained what quantity of the hydrogene he had discharged from his lungs, and what proportion it bore to the other gases expelled at the same time. From these data he determined what quantity of hydrogene was still retained, and supposing it to have been uniformly mixed with the whole air in the lungs, he easily calculated the absolute volume of air still remaining in them. Making the due allowances for the expansion, which it would experience from the heat of the body, he supposes that the capacity  
of

(a) *Researches*, p. 433.



of his lungs in a state of complete expiration amounts to no more than 41 cubic inches. (a)

It is difficult to reconcile these estimates with those of Dr. Menzies, which were apparently deduced from accurate and well contrived experiments. With respect to Mr. Davy's method of determining the bulk of a single inspiration, we may remark in general, that the quantity of air taken into the lungs varies so much at different times, and depends in so great a degree upon the will, that other circumstances being the same, those experiments will stand the best chance of accuracy in which an average is taken from a considerable number of inspirations; in this particular the experiments of Dr. Menzies possess a decided superiority. There is also another circumstance to be noticed with respect to the peculiar apparatus employed by Mr. Davy, which may be suggested as a probable cause of inaccuracy. He informs us that he attempted to breathe in the mercurial air-holder in such a manner that the respiration might require no unusual effort, and he concludes, that in this case the function would be exercised in the same manner as in its natural state. But in breathing into this apparatus,

(a) *Researches*, p. 409.

ratus, notwithstanding the nice adjustment of its parts, some degree of resistance is to be overcome, which does not exist in ordinary respiration, and therefore if the same degree only of exertion be employed, a less quantity of air will certainly be emitted from the lungs. It would require a number of successive respirations greater than appears to have been employed by Mr. Davy, before any painful sensation will arise from this impediment; uneasiness in the organs of respiration is produced more readily by an alteration in the quality than the quantity of air received into them.

The estimate which Mr. Davy has formed of the capacity of the lungs, in the state of complete expiration, differs so materially from other direct experiments, and is so incompatible with the ideas which are derived from a consideration of the anatomical structure of the thorax, that it is impossible to avoid suspecting the existence of some error or fallacy in the process which he employed. The only conjecture which occurs to me, in any degree likely to remove the difficulty, is the supposition, that the hydrogen was not diffused uniformly through all the cavities of the lungs; a supposition which is in some degree countenanced by the consideration of the different specific gravities of the gases, and which derives confirmation

confirmation from the experiments of M. Jurine, of Geneva. He received the air of one expiration into four different vessels, and he found the different portions of air to exhibit different chemical properties. According to the order in which they had been expelled from the lungs, they contained a greater proportion of oxygene, and a smaller of carbonic acid. Hence it may be conjectured, that in the experiment of Mr. Davy, the hydrogene which he had inspired, had not mixed itself with the air contained in the vesicles, in the same proportion as in the larger branches of the trachea, consequently the air upon which the experiment was made, would contain a greater proportion of hydrogene than the average of the whole air contained in the lungs, and the estimate of their capacity, taken from the quantity of hydrogene, would be necessarily below the truth. (a)

The experiments which I have related of Goodwyn, Menzies, and Davy, are the most accurate that have been performed upon the mechanism of respiration; it will however be proper to notice the estimates of some other authors, even though we may find them to be less satisfactory.

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An interesting account is given in the *Encyclopedie Methodique* (a) of some experiments performed by M. Jurine, on the chemical change produced upon the air by respiration in different conditions of the body. In the course of the detail, he is led to mention the bulk of a single inspiration, which he estimates at 20 cubic inches; this the editor, M. Hallé, considers as too large a quantity; but there is no mention made of the reasons upon which the opinions either of M. Jurine, or of M. Hallé are founded. Mr. Kite, in his essay on submerſion, ſtates the quantity of air received by an ordinary inſpiration to be 17 cubic inches, (b) and M. Delametherie, the learned editor of the *Journal de Phyſique*, reduces it to a much leſs quantity, (c) in neither inſtance, however, are we informed of the experiments by which theſe reſults were obtained. Mr. Abernethy employed an inverted jar filled with mercury, and by reſpiring as much as poſſible in the ordinary manner, he expelled from the lungs 12 cubic inches. (d).

Mr. Coleman, in the courſe of his work on ſuſpended reſpiration, is led to compare the capacity

(a) *Article Medecine*, t. i. p. 493, & ſeq. (b) p. 47.

(c) *Journal de Phyſique*, t. xlvi. p. 108.

(d) *Essays*, p. 142.

city of the lungs after drowning, with their capacity in a state of health, and thus to discover the diminution which the cavity of the thorax experiences in a state of complete expiration. He conducted his experiments by applying a ligature about the trachea of an animal which he had previously drowned, he then detached the lungs from the body, and pressed out all the air into an inverted jar of water. He afterwards inflated the lungs, and noted the quantity of air they were capable of receiving when fully distended; these two quantities he compared together (*a*). The results of his experiments were very different from what might previously have been expected; the lungs of a cat, which when inflated were capable of holding 16 drachms of air, after drowning, were found to contain only half a drachm; and in a dog, which he killed by hanging, the proportions were still more extraordinary, nearly as forty-three to one. We have no reason to doubt either the faithfulness with which these experiments are related, or the accuracy with which they were performed, yet, as the lungs always exactly fill the cavity of the thorax, they cannot be reduced in capacity without a proportionable reduction in the size of the cavity containing

(*a*) p. 91. & seq.



containing them, and it seems almost inconceivable, that so enormous a difference could take place in the capacity of the thorax of the same animal. (a) We may therefore conjecture that there exists a source of fallacy arising from the nature of Mr. Coleman's experiments. I shall point out some circumstances which may, in part at least, remove the difficulty.

When an animal is killed by interrupting the passage of the air into the bronchial vessels, whatever may be considered as the immediate cause of death, it is found, that a considerable portion of the air which is left in the lungs is converted into carbonic acid gas. According to the plan upon which Mr. Coleman conducted his experiments, a part, at least, of this gas must necessarily have been absorbed by the water through which it was received, and upon which it was lodged; consequently his estimate of the quantity of air left in the lungs will be less than the truth. This absorption of carbonic acid gas may tend in some degree to reconcile the apparent contradiction in Mr. Coleman's experiments, but the circumstance which I shall next point out is more extensive in its operation, and will have a much more important effect upon our reasonings.

When

(a) Note 15.

When an animal is in the act of drowning, the sense of suffocation which is experienced produces a violent effort to expire. All the muscles which can conduce to this effect are brought into strong action, and contracted to the utmost, in order to expel from the lungs as large a quantity as possible of the air which is contained in them. These muscles, however, becoming exhausted by their efforts, are quickly relaxed, and the thorax returns nearly to its natural dimensions. In consequence of the situation in which the animal is placed, the external air has no access to the lungs, and on this account the air which remains in them becomes considerably rarefied. Owing to this circumstance, a portion of the mucous fluid which lines the membrane of the vesicles is converted into an aqueous gas, which is mixed with the rarefied air, and fills the cavities of the lungs. (a) The uneasiness of the animal continues to encrease, and after a short time, a second violent effort to expire ensues, and a part of this rarefied air, together with some of the aqueous vapour, is forced out. This process is repeated until a large proportion of the air originally contained in the lungs is excluded from them, and the powers of the animal being completely exhausted,

(a) Note 16.

hausted, the respiratory organs cease to act. It is obvious on the first view of the subject, that in the method of experimenting employed by Mr. Coleman, all the aqueous vapour was entirely disregarded in his estimate of the contents of the lungs. When they were removed from the thorax and exposed to the pressure of the atmosphere, the greatest part of this vapour would be immediately condensed, and any portion of it which still retained its elastic form, would be totally destroyed in its passage through the water contained in the inverted jar (a). It may be farther remarked, that owing to the circular shape of the bronchia and vesicles, and more especially to the cartilaginous texture of the former, it is impossible by mere pressure to expel all the air which they contain. When they have once been rendered pervious by inspiration, no degree of compression can render them specifically heavier than water; it is even doubtful whether this effect can be produced by placing them in the vacuum of the air pump: (b) but it does not appear that these precautions were employed in the experiments now under consideration.

From the circumstances which I have pointed out,

(a) Note 17.

(b) *Petit Mem. Acad. 1733, Boer Prælect. t. v. p. 458.*

out, it is evident that Mr. Coleman has estimated below the truth the capacity of the lungs in their state of complete expiration, and there is reason to apprehend, that in determining their capacity in the ordinary condition of inspiration, he has fallen into the opposite error. In order to ascertain this point, he detached the lungs from the body, and after distending them with air, measured their contents by pressing it, as in the former experiment, into a graduated vessel filled with water. From the structure of the lungs, and the elasticity of their substance, it is not improbable that when they were in this manner fully distended, they would be stretched to a size beyond what they occupied while contained in the cavity of the thorax. (a)

These remarks upon the method by which the process of respiration is effected, will shew, that though physiologists are agreed as to the mechanism by which the air is received into and sent out of the lungs, yet that with respect to their relative capacity in the different states of respiration, their opinions and experiments are much at variance.

Before we can arrive at a perfect knowledge  
of

(a) Boer. Instit. § 603. Blumenbach. Instit. Phys. p. 11.

of this part of the subject, it will be necessary to ascertain precisely the 5 following points. 1st. The quantity of air received into and sent out of the lungs in an ordinary act of respiration. 2d. The proportion which this bears to the quantity of air contained in the thorax after an ordinary expiration, which may be considered as the natural condition of the thorax. 3d. The quantity of air which can be still expelled after an ordinary expiration; and 4th. The proportion which this bears to the air still left in the lungs, which will measure their state of complete expiration. Lastly. The quantity of air which can be taken into the lungs by the greatest effort of inspiration.

The first of these points is, I apprehend, ascertained by the experiments of Dr. Menzies. Though the calculations of other physiologists are considerably different, yet, there seems no obvious source of inaccuracy in the processes which he employed, and the coincidence of the different experiments with each other is certainly too remarkable to be merely the effect of accident. Respiration is an act so much influenced by external causes, that we can never expect to attain any certainty as to the quantity of air ordinarily

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received



received into the lungs, without taking the average of a considerable number of inspirations. By means of the allantoid, Dr. Menzies was enabled to compare together 56; in this respect alone his experiments possess a decided superiority over those of any other physiologist. The coincidence which took place between the results of his experiments performed with the allantoid, and those in which a man was placed in a vessel of warm water, convey to the mind an almost irresistible conviction of their truth. We may therefore conclude, with respect to the first of these points, that 40 cubic inches is the quantity of air employed in an ordinary act of respiration. (a)

The proportion which an ordinary expiration bears to the quantity of air still left in the lungs has been variously estimated. The calculations of the older writers upon this subject seem to have been very incorrect. It appears that in performing their experiments they removed the lungs from the thorax, and then observed what quantity of air they were able to force into them, in order to their complete distention; but the uncertainty of this mode of proceeding is sufficiently obvious. Dr. Menzies estimated the medium capacity

(a) Note 18.

capacity of the lungs in a state of ordinary expiration at 179 cubic inches; I have already stated the reasons which induce me to consider this quantity as under-rated, and to conceive that the lungs in their natural condition contain about 280 cubic inches of air.

The experiments and observations which have been made to determine the quantity of air left in the lungs after a complete expiration, have been pretty fully detailed, and it appears, that notwithstanding the ingenuity and labour which have been exercised upon this point, it must still be considered as undetermined. Upon the whole, Dr. Goodwyn's estimate of 109 cubic inches is the nearest approximation to the truth, though, probably, not altogether correct.

From the above data it may be estimated, that by each ordinary expiration  $\frac{1}{7}$  part of the whole contents of the lungs is discharged, and that by the most violent expiration, somewhat more than  $\frac{4}{7}$  of the air contained in them is evacuated. Supposing that each respiration occupies about 3 seconds, (a) a bulk of air nearly equal to three times the whole contents of the lungs will be

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expelled

(a) Haller, viii. 4, 29. Thomson's Chemistry, v. iv. p. 486.

expelled in a minute, or about 4114 times their bulk in 24 hours. The quantity of air respired during the diurnal period, will be 1152000 cubic inches, or  $666\frac{1}{2}$  cubic feet.

CHAP.

## CHAP. IV.

*Inquiry into the cause of the first Inspiration,  
and of the Alternation of Inspiration and Ex-  
piration.*

IN viviparous animals respiration commences immediately after birth, when the circulation which had been previously carried on between the mother and the foetus is interrupted. (a) The manner in which the commencement of this important action is effected, and the change which is then produced in the animal constitution, so as to render this function absolutely necessary for the continuance of existence, have been frequently made the subject of investigation. The latter of these inquiries is the foundation of the celebrated problem which was proposed by Harvey for the consideration of his contemporaries. "Quomodo nempe embryo," he asks, "post septimum mensem in utero matris perseveret? cum

(a) Note 19.

“ cum tamen eo tempore exclusus statim respiret ;  
 “ imo vero sine respiratione ne horulam quidem  
 “ supereffe possit ; in utero autem manens ultra  
 “ nonum mensem, absque respirationis admini-  
 “ culo, vivus at sanus degat.” (a) This ques-  
 tion may be stated still more generally ; why is  
 the animal which has once respired, under the  
 necessity of continuing the respiration without  
 intermission ; when, if the air had never been  
 received into the lungs, the same animal might  
 have remained for some time without exercising  
 this function ?

Several of the older physiologists proposed answers to this question, but they were founded either upon theories which are now universally discarded, or upon mistaken notions respecting the use of respiration. Dr. Whytt's hypothesis is the first which I shall notice ; it is not indeed free from objections, but it deserves attention on account of the reputation which it long maintained in some of the most distinguished schools of medicine.

This author commences his inquiry into the cause of the first inspiration by remarking, that  
 before

(a) *Harveii Exerc. de gen. anim.* p. 361. *Haller*, viii. 5, 1.



before birth, the blood of the mother is properly prepared for the use of the foetus, but that when the communication between them is interrupted, it becomes necessary for the young animal to perform the requisite alteration in its fluids, by means of its own respiration. As the uneasy sensations of hunger and thirst excite in us a desire of food, when it is wanted for the nourishment of the body, so immediately after birth, from the want of fresh air in the lungs, there arises an uneasy sensation about the thorax, which may be considered as the appetite for breathing. This appetite he considers as natural to the newly born animal, and the expansion of the cavity of the thorax is effected by the sentient, active principle, in order to prevent the fatal consequence which would ensue from the deficiency of air in the lungs. Dr. Whytt proceeds to observe, that this appetite for air commences only at birth, because, in consequence of the struggles of the foetus at this period, its circulation will be considerably quickened, and the lungs will receive a larger share of blood than previously passed through them; this stimulates them into action, and seems to be the immediate cause of the appetite for breathing. He farther urges, that the circumstance of the animal being now immersed in a medium which is proper for respiration, and  
 having

having the air applied to the face, mouth, and nostrils, may cause it to attempt this new action of the thorax. Upon the whole he considers breathing "as owing to a peculiar sensation of the body, which determines the mind or sentient principle to put certain muscles or organs in motion."

With respect to the problem proposed by Harvey, which was mentioned above, he observes it "to be of so very easy solution, that it is not a little surprizing, that many physiological writers should have attempted it in vain." When the foetus has once breathed, he remarks, that the blood which formerly passed through the foramen ovale & ductus arteriosus, now passes through the vessels of the inflated lungs, and will ever after continue to flow through them. If, therefore, the respiration be discontinued, the blood will stagnate in these vessels, and the animal will be suffocated, because, according to his hypothesis, the alternate motion of the lungs in respiration, is absolutely necessary to carry on the circulation of the blood through them. (a)

Haller conceives that the foetus, having before birth been in the habit of swallowing a portion of

of the fluid in which it is immersed, after it leaves the uterus, continues to open its mouth for the reception of nutriment, when a portion of the air with which it is now surrounded, is necessarily taken into the lungs. The vesicles being thus expanded, permit the blood to pass through the pulmonary vessels, which was formerly transmitted by the foramen ovale and ductus arteriosus. The lungs then acquire the condition of those of other breathing animals, and like them require a regular supply of fresh air to prevent the blood from stagnating in its passage from the right to the left side of the heart. (a)

The following solution of this difficulty is proposed by the celebrated author of the *Zoonomia*. Dr. Darwin imagines that the power of deglutition is acquired by the foetus before it leaves the uterus, in consequence of its repeated attempts to swallow a part of the liquor amnii, in which it is immersed; but the act of deglutition is of so different a nature from that of inspiration, that he thinks this latter cannot be acquired before birth. When the circulation is interrupted between the mother and the foetus, that disagreeable sensation is experienced by the infant, which arises

arises from the want of fresh air ; all the muscles of the body are put in motion to relieve this anxiety ; and among others, those of the breast, ribs, and diaphragm. The action of these being found to answer the desired end, “ respiration is “ discovered,” and the same action is ever after repeated when the disagreeable sensation recurs.

(a) It will be unnecessary to enter into a formal refutation of these theories ; though they may be entitled to the praise of ingenuity, they are evidently built upon the assumption of principles which are at least doubtful, if not untenable. The question concerning the commencement of respiration is so intimately connected with the ideas which we entertain respecting its uses, that we can scarcely expect a complete solution of the difficulty until we have previously ascertained the purposes which this function serves in the animal œconomy. There are, however, some peculiarities in the structure and mechanism of the respiratory organs of the foetus, which it will be proper to notice in this place.

The lungs of an animal which has never respired, are of a firm and compact texture, and so condensed, as to have a specific gravity greater than that of water ; whereas, no mechanical pressure



ture is found sufficient to evacuate them, so far as to cause them to sink in water, after they have been once inflated. (a) In this state the pulmonary arteries and veins receive only  $\frac{1}{2}$  part of the quantity of blood which circulates through them, after they have been distended by the act of inspiration. (b) The cavities of the bronchia and air vesicles are nearly obliterated, and that part of them which remains pervious, is filled with a peculiar mucous fluid. (c) As the lungs are in all cases contiguous to the parities of the thorax, it is obvious, that the size of the cavity containing them, while in this condensed state, must be proportionably contracted. Accordingly we find that the arch of the ribs is depressed, by which means the sternum is made to approach nearer to the spine, the diaphragm is pushed up into the higher part of the chest, and its concavity towards the abdomen is considerably greater than it is ever found to be in an animal which has once respired. (d)

The space occupied by the lungs themselves is still farther diminished by the great size of the heart in the foetus, and by the interposition of the thymus

(a) Boer. *prælect. t. v. Pars 2a. p. 457.* Petit, *Mem. Acad.* 1733. (b) Haller in Boer. *Prælect. t. ii. p. 172.* (c) Petit, *ubi supra.* (d) Senac, *Mem. Acad.* 1724.



thymus between the laminæ of the mediastinum.

(a) This contracted state of the thorax is in part also produced by the posture of the foetus before birth; the head is bent forwards, so that the chin rests upon the breast, the spine is arched, and the knees are raised up to meet the face. (b) The abdominal viscera are therefore pushed strongly against the diaphragm, and so far increase its curvature, that a considerable portion of them is forced into the region of the thorax: the liver, which in the foetus is unusually bulky, (c) is entirely concealed by the ribs. As soon however as the animal leaves the uterus, its position is changed, the limbs and the trunk are straightened, and the pressure consequently removed from the thorax and abdomen; the elasticity of the cartilages of the ribs being at liberty to act, raises the arch of the thorax, and thus encreases the distance between the sternum and the spine; the liver and the rest of the abdominal viscera fall down into their proper situation, and permit the diaphragm to descend and assume the curvature which is natural to it after birth.

While

(a) Haller, xxix. 4. 33. do. in Boer. Præl. t. v. Pars 2a, p. 459.

(b) Harveii Exer. de gener. anim. p. 353. Denman's Midwifery, v. i. p. 293.

(c) Haller, ubi supra.

While these changes are going forward in the relative position of the contents of the thorax and abdomen, the increased size of the former cavity causes the air to rush through the open mouth of the animal into the bronchial vesicles; whence the blood, meeting with less resistance to its passage through the vessels of the lungs, than through the foramen ovale is propelled along the pulmonary artery. The respiratory organs are then in their natural condition, or in the state of moderate expiration, and the necessity for an inspiration depends upon the same cause, which renders the alternation of expiration and inspiration essential to the future existence of the animal.

It appears therefore that the first degree of expansion which is produced in the lungs of the foetus, is not effected by muscular contraction, but depends merely upon the removal of external pressure, which permits the different parts of the thorax and abdomen to assume their natural position; the consequence of this new situation of the viscera is an encrease of the capacity of the chest, and this space is immediately filled with a portion of the surrounding air. Any farther encrease of the size of the thorax must be effected by the contraction of the diaphragm and intercostals; perhaps in strict propriety this muscular

cular contraction ought to be considered as the first beginning of the action of respiration.

Nearly allied to this investigation concerning the commencement of respiration, is a question which has also been the subject of much discussion among physiologists, viz. what is the immediate cause of the successive alternations of inspiration and expiration ?

The solutions which have been attempted of this question, have been no less numerous than those respecting the cause of the first inspiration, but as the subject is in itself perhaps more obscure, so we shall find the explanations which have been advanced, still less satisfactory. I shall mention the opinions of Haller and Whytt, which are the least exceptionable, and are still received by many respectable physiologists.

Haller, proceeding upon the supposition that in expiration the passage of the blood through the lungs is impeded, observes, that a pressure upon the brain, and a reflux of the blood through the veins, will be produced from this circumstance. Hence arises a necessity for encreasing the capacity of the thorax, in order to remove the compression of the pulmonary vessels, and in consequence

consequence of a painful sense of anxiety or suffocation, proceeding from a partial stagnation of the blood, the will calls into action the muscles of inspiration, which remove the impediment to the transmission of blood through the lungs, and consequently the painful feeling which was caused by the obstruction. The same uneasy sensations are however produced by inspiration, when protracted beyond a certain period; the muscles therefore, which were acting to enlarge the thorax, cease to contract, their relaxation is followed by a state of expiration, which continues until the resistance to the passage of the blood recurs, and brings back the consequences stated above. Haller considers the action of the muscles of respiration as altogether voluntary. (a)

Dr. Whytt offered an explanation in many respects similar to that proposed by Haller. He conceives, that the passage of the blood through the pulmonary vessels, is impeded by expiration, and that a sense of anxiety is produced from this cause; this unpleasant feeling acts as a stimulus upon the nerves of the lungs, and excites the energy of the sentient principle, which by causing a contraction of the diaphragm, enlarges the

(a) *El. Phys.* viii. 4. 18, 27, 28. *Not. ad Boer. Prælect.* t. v. p. 120.

the thorax, affords a free passage to the blood, and thus removes the uneasy sensation ; when the stimulus no longer existing, the muscles cease to act. (a)

The question under consideration embraces two distinct subjects of inquiry ; we have in the first place to ascertain the cause of the necessity for the admission of fresh air into the lungs, and secondly, we have to investigate the connexion which subsists between this cause and the contraction of the diaphragm. The first of these is so intimately connected with the opinions which we may adopt respecting the use of respiration, that the farther examination of it must be necessarily postponed to a subsequent part of the essay. I shall therefore conclude this chapter, with a few observations upon the latter part of the inquiry, viz. the nature of the connexion which subsists between the peculiar state of the lungs produced by an interruption to the respiration, and the contraction of the diaphragm.

As the alternate motion of the thorax in respiration, proceeds without interruption in all the different conditions of the body, it is improper, in attempting to assign a cause for it, to have

(a) *Whytt on Vital Motions, sect. 8.*



have recourse to any effects which are produced only in extraordinary circumstances; we cannot therefore establish our hypothesis, upon a state of the lungs which takes place only in long continued or laborious respiration. On this account the first position in the hypotheses of Haller and Whytt is inadmissible, since it supposes the existence of a peculiar state of the pulmonary circulation, which does not appear to take place, unless where the muscles of respiration have been acting in the most violent manner. It may be doubted, whether in ordinary cases an uneasy feeling of suffocation actually occurs, as we are only sensible of this effect, when the lungs are compressed or distended in an unusual degree, or when the air has been too long retained in them without alteration. Admitting however the existence of this uneasy feeling, and assuming it as the efficient cause of the succeeding enlargement of the thorax, we shall hereafter endeavour to assign a more probable cause for it than the stagnation of the blood in the lungs.

One of the properties which, in an especial manner, distinguishes the living body from every other kind of matter, is its irritability, or the power which certain parts of it possess of contracting themselves under peculiar circumstances.

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This power resides in the muscular fibres, and is never exerted unless when called into action by the application of a stimulus. Stimuli are external or internal; under the first denomination are comprehended a great variety of bodies which have scarcely any property in common, except their effect upon the muscular fibre. The internal stimuli consist of affections originating in the nervous system, of the passions and emotions of the mind, and in a remarkable manner of the exertions of volition. The motions which result from these contractions have been variously arranged, either according to the cause producing them, or to the purposes which they serve in the animal œconomy. One of the most common, and at the same time, one of the most important divisions, is into voluntary and involuntary motions, an arrangement obviously founded upon the share which the will has in their production. There are a very considerable number of animal motions, which are intermediate between the two extremes; these are to a certain extent produced by the application of external stimuli, and consequently independent of volition; yet within certain limits we have it in our power to encrease or restrain them. Animal motions which are produced by external stimuli, are distinguished from each other, according to the manner

manner in which the stimulus is applied ; in general the motion is excited in the part upon which the stimulus immediately acts, while in other instances, the stimulus is applied to a part which has little apparent connexion with that in which the motion is ultimately produced. Almost all the voluntary actions are of this complex description, and some of those also which are produced by the agency of external stimuli.

There is a class of motions of very peculiar importance in the animal œconomy, which have obtained the name of instinctive actions ; they seem to originate primarily from the operation of external stimuli, but the effect is generally produced in a part different from that to which the stimulus was applied. In the first instance they appear to be totally independent of volition, though, after they have subsisted for some time, the will acquires the power of modifying their extent and duration. The process of deglutition, the motion of the eye lids, and the evacuation of the intestinal canal, may be adduced as instances of this species of animal action. Here, the animal immediately after birth, produces an effect depending upon the co-operation of a complicated system of muscular contractions ; the efficient cause, appears to be a stimulating

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body

body applied to a part often remotely connected with the muscles which are excited into action, where the animal, must of necessity, be entirely ignorant of the proposed effects, and perhaps scarcely conscious of their existence. (a)

To apply these observations to the action of the thorax : it appears, that when the blood has remained for some time in the pulmonary vessels, without the access of fresh air, it undergoes a change, either in its composition or its distribution, which causes it to act as a stimulus to the diaphragm, and excites this muscle to contract. The connexion between this change in the state of the blood, and the contraction of the diaphragm, we are unable to explain, and we can only recognize it from its effects ; whether it be produced by an alteration in the chemical properties of the blood, or whether it depend upon an accumulation or deficiency of it in any part of the thorax, whether the action be directly upon the diaphragm or its nerves, or whether it be brought about by the intervention of any neighbouring organ, are questions which the present state of our knowledge will not permit us to answer. But in whatever manner the contraction  
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of the diaphragm be produced, the consequent enlargement of the thorax admits the access of air into the pulmonary vesicles; the blood is thus enabled to undergo the necessary change, and the state which caused the contraction of the diaphragm no longer existing, this muscle becomes relaxed, and expiration ensues. If the justice of these remarks be admitted, we may divide respiration into three distinct species, each of which originates from a different principle of action. What has been just described, which may be considered as the ordinary process of respiration, is the first species: in this the effect seems to depend upon the action of a stimulus applied to an irritable part. When, however, from any accidental or morbid cause, the regular train of actions is interrupted or impeded, so as to prevent the blood from undergoing the necessary change in its composition, an uneasy sensation arises, and we are urged by an instinctive feeling to encrease the contraction of the intercostals and diaphragm, and to employ the external muscles of the thorax. These motions, which produce the second species of respiration, must however, be still considered as independent of volition, because they take place to the fullest extent in those cases where we cannot suppose the powers of the will to be exercised either directly,  
or



or by the intervention of association. As however we gradually advance in life, we acquire a degree of voluntary power over the muscles which are subservient to respiration, and can so far counteract the irritative, or instinctive motions, as very considerably to accelerate or retard their action. In this manner is produced the third species of respiration, or that which depends upon the exercise of volition. (a)

(a) Note 22.

PART

## PART II.

### THE DIRECT EFFECTS OF RESPIRATION.

*THE direct effects of respiration may be conveniently arranged under three divisions; the mechanical effects produced by the dilatation and contraction of the thorax; the change produced in the inspired air; and the alteration which the blood experiences during its passage through the capillaries of the lungs.*

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#### CHAP. I.

##### *The Mechanical Effects produced by the Dilatation and Contraction of the Thorax.*

THE process by which the cavity of the thorax is alternately dilated and contracted, and a portion of the air of the atmosphere received into and emitted from the bronchial vessels of the lungs, has been already described. I propose in the present chapter to examine what effects will be produced upon the contiguous parts, by the perpetual change of bulk which the thorax experiences. The older physiologists, whose knowledge respecting the function of respiration, was  
for

for the most part, very limited, and who were entirely ignorant of the nature of the chemical changes produced upon the air and the blood, were led to direct their attention more particularly to its mechanical effects. The question, whether the blood was transmitted through the lungs with more ease during the state of inspiration or of expiration, was minutely discussed, and a number of experiments were performed to decide upon this point ; but they will be found to apply rather to what takes place in extreme cases, than to the ordinary process of respiration. When attempts have been made to resolve this question by a direct experiment upon a living animal, the loss of blood, the pain which was necessarily inflicted, and more particularly the agitation into which the organs of respiration were thrown, render the results of comparatively little value, so far as they relate to the ordinary action of the thorax ; though we certainly learn from them, that the blood is transmitted through the lungs with more facility in the extreme states of inspiration than of expiration. In an experiment performed by Dr. Hales, where a tube was inserted into the crural artery of a horse, the blood was evidently propelled to a greater height when the animal sighed deeply, which must be attributed to the increased capacity of the lungs, permitting the  
blood

blood to pass more freely through the pulmonary vessels. (a) But though from this experiment, and others of a similar nature related by Haller, it seems proved, that in these extraordinary cases, the passage of the blood through the lungs is affected by their state of distention, we must not conclude that this effect is produced in the usual alternations of respiration. There are indeed strong reasons in support of the contrary opinion. In the healthy state of the body we respire about twenty times in a minute, while the average velocity of the pulse is about eighty, so that the heart contracts four times during one act of respiration, (b) and consequently it must receive its supply of blood from the lungs in all the different conditions of distention to which they are subject, yet we do not find that the pulse is affected, either with respect to its strength or its velocity. (c) It is difficult to produce any effect upon the pulse by the most powerful voluntary efforts of inspiration or of expiration, yet in these cases, the cavity of the thorax certainly undergoes a much greater change in its dimensions, than that which it experiences in its ordinary action.

The contrary doctrine was however, maintained

(a) *Statistical Essays*, v. ii. p. 6. (b) *Haller*, viii. 4. 29. *Boer. Præl. t. v. p. 128.* (c) Note 23.

tained by Haller; he supposes, that during expiration, the bronchia are, to a certain degree, plaited or folded up, but that when the lungs are inflated, these folds are opened, and the blood vessels rendered more easily permeable. (a) This opinion, which was generally adopted by Haller's contemporaries, (b) and by many of them exaggerated in a most extravagant degree, (c) seems to have arisen from the following circumstances. 1. They greatly over-rated the change of bulk which the thorax experiences in the different stages of respiration. If we average the contents of the thorax at 280 cubic inches, and the bulk of a single inspiration at 40, we shall have its cavity increased or diminished by  $\frac{1}{7}$  part only; whereas the older physiologists conceived, that by every act of inspiration, the cavity of the chest was doubled, quadrupled, or increased in even a still greater proportion. (d) 2. In the experiments and observations which were made upon the dead body, they considered only the effects which are produced by a complete evacuation of the lungs, and then applied these to explain the effects of ordinary respiration. (e) 3. Haller and his friends appear to have been misled by

(a) *El. Phys.* vi. 4. 10; viii. 4. 11; viii. 5. 21. & alibi; notæ ad Boer. *Prælect. t. ii. p. 176.* (b) Note 24. (c) *Bell's Anatomy*, v. ii. p. 188. & seq. (d) Haller, viii. 4. 11 (e) Note 25.



by the experiments which they performed upon living animals; they perceived that the transmission of blood through the lungs was influenced by their different states of distention, but they neglected to make the due allowance for the unnatural situations in which the subjects of these experiments must necessarily have been placed. Dr. Goodwyn, in order to controvert this opinion respecting the state of the lungs after expiration, produced an artificial hydrothorax in dogs, by introducing a quantity of water between the pleuræ. He found in several instances, when he had filled nearly  $\frac{1}{2}$  of the cavity of the thorax, that the passage of the blood was not apparently retarded, though the respiration was rendered laborious. (a) Upon the whole I think there is reason to conclude, that in the usual act of respiration, the blood is transmitted through the lungs at all times with nearly equal facility, and that it is only in extreme cases that the retardation described by Haller can be supposed to take place. (b) If any considerable impediment had been opposed to the entrance of the blood into the pulmonary vessels during the state of expiration, it must necessarily have produced upon the pulse a corresponding effect, which would have been perceptible to the touch. (c)

This

(a) Goodwyn, p. 45. Note 26. (b) Note 27. (c) Note 28.

This doctrine of the older physiologists respecting the transmissiion of the blood through the lungs, gave rise to an opinion, which was almost universally adopted by them, that the motion of the thorax in respiration was an important agent in promoting the circulation of the blood. This hypothesis will be considered more fully, when we come to treat of the uses of respiration; I shall in this place only remark, that the same considerations which lead us to conclude that the lungs are nearly alike permeable to the passage of the blood in all the stages of ordinary respiration, must prove that their motion can have little or no effect in accelerating its progress through their vessels.

The œsophagus passes through the muscular portion of the diaphragm, and is so interlaced by the crossing of its fibres, that it seems as if the tube must necessarily be compressed by their contraction. (a) Hence it has been supposed, that during inspiration the passage to the stomach will be contracted or entirely obstructed; but here, as in the former case, it is doubtful whether this effect be produced, except in the more violent efforts of inspiration.

Some

(a) *Haller*, viii. 1. 36. xviii. 4. 9. *Senac*, *Mem. Acad.* 1729. *Winslow's Anat.* Sect. 3. Art. 13. *Bell's Anat.* v. i. p. 326.

Some of the older physiologists imagined that important effects would result from the action of the diaphragm, upon the aorta and vena cava. The aorta, however, descends into the abdomen, between what are called the crura of the diaphragm, and seems to be so well protected by the arching of these muscular appendages, as to be altogether defended from the effects of compression. (a) There seems still less probability of strangulation being produced in the vena cava, as it not only pierces the diaphragm in its tendinous part, but passes through a large opening, the edges of which are furnished with a peculiar structure, which would seem to prevent any alteration in its shape or dimensions. (b) Yet notwithstanding this structure, Haller observed in the course of his experiments, that certain positions of the diaphragm so far compressed the vena cava, as to impede the passage of the blood. (c) But we may conjecture that this effect was produced by the unnatural circumstances in which the animal was placed during the experiment; we may also attribute something to the difficulty of making accurate observations upon animals suffering

(a) Senac, & Bell, *ubi supra*.

(b) Winslow, Senac, & Bell, *ubi supra*.

(c) Haller, vi. 4. 10; viii. 1. 36; viii. 5. 23.

ing extreme torture, and convulsed by the near approach of death.

The older anatomists ascribed very important effects to the successive compression and relaxation of the nerves which pass through the diaphragm, or along its surface. The alternate motions of the thorax, and even of the heart itself, were ascribed to this circumstance, but later observations have proved that it was inadequate to the proposed effect, and have also pointed out causes, which are more agreeable to the analogy of the animal œconomy. The par vagum, and great sympathetic nerves, both pass through the smaller muscle of the diaphragm, (a) and it has been conjectured, that by the alternate contractions and relaxations of this organ, they are compressed at regular intervals, and thus the transmission of the nervous energy is interrupted, so as to produce the vermicular motions of the stomach and intestines. I have already noticed the hypothesis of Dr. Martine, who conceived, that the compression of the phrenic nerve by the lungs, when in a state of inspiration, was sufficient to account for the succeeding relaxation of the diaphragm,

(a) *Haller*, viii. 1. 35, & seq. *Winslow's Anat.* Sect. 6. 137, 400.

phragm, and consequent emission of air from the thorax.

The alternate enlargement and contraction of the bulk of the thorax, in consequence of the change in the diaphragm, must necessarily produce a corresponding change in the position of all the abdominal viscera. The agitation and pressure which they will from this cause experience, has been generally supposed to be instrumental in propelling the blood along their veins; (a) but this effect, if it exist at all, has been much over-rated. From the experiments of Dr. Menzies, in which the body was immersed in water for the purpose of ascertaining the quantity of air employed in a single inspiration, it was found that the increase of bulk which the body experiences, is exactly equal to the volume of the air received by the lungs. (b) Hence we must conclude, that the cavity of the abdomen is at all periods of the act of respiration of an equal capacity, the increase of bulk which the abdomen would acquire, in consequence of the relaxation of its anterior muscles, being exactly balanced by the contraction of the diaphragm, and consequently the pressure exercised upon the viscera, must

(a) *Haller*, viii. 5. 23.

(b) *Menzies*, p. 24, & seq.



must be altogether, or nearly similar in all the different conditions of the thorax. But a uniform pressure upon the vessels can have no effect in accelerating the flow of the blood; the veins are in this case reduced to the state of rigid tubes, and their contents can only be propelled by the momentum which they possessed previous to their arrival at this part of the circulation. An increase of pressure under these circumstances, must rather retard than promote the flow of the blood.

This view of the subject is countenanced by the deficiency of valves in these vessels, a circumstance in which the veins of the abdominal viscera differ from those of most other parts of the body. (a) The vessels which lie near the surface, or contiguous to the great muscles, and which consequently are at all times peculiarly exposed to the effects of irregular pressure, are plentifully furnished with semilunar valves. When, therefore, by an extraneous cause, the capacity of any part of a vein is diminished, the blood which it contained must necessarily be driven forward towards the heart, and upon the removal of the compression, the vessel regaining its  
natural

(a) Haller, ii. 2. 18, 21...

natural capacity will afford a more easy admission to the succeeding column of blood. But the acceleration which is produced in this case, depends entirely upon the presence of the valves; when, therefore, we find a system of venous vessels in which they are wanting, we may infer that the veins in question are not much exposed to the effects of pressure, and that when pressure is applied, it will not accelerate the motion of the fluids which they contain. (a)

Haller insists strongly upon the effects which are produced in the liver by the contraction of the diaphragm. He conceives that it is a principal agent in propelling the blood along the hepatic veins, and more particularly in pressing the bile out of the gall bladder: he supposes that this organ is not furnished with any muscular fibres, and that consequently its contents can only be evacuated by external pressure. There seems every reason to conclude that the effects which he describes will be produced in the more violent and sudden motions of the diaphragm, particularly those which occur in the act of vomiting. (b)

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(a) Haller, iii. 2, 3; & viii. 5. 23. *Bell's Dissections*,  
2. i. p. 48.

(b) Haller, viii. 5. 23. & xxiii. 3. 29.

The alternate action of the thorax is also supposed to exercise a considerable degree of compression upon the lacteals, and consequently to promote the flow of the chyle. (a) The remarks made above with regard to the effect of the diaphragm upon the veins of the abdominal viscera, apply to the present case, so far as respects the uniformity of the pressure in the ordinary act of respiration. But the lacteals differ from the veins of the viscera in being furnished with numerous valves, (b) it is therefore probable that the chyle will be propelled towards the thoracic duct, in the more violent action either of the diaphragm, or the muscles of the abdomen, and still more in those voluntary exertions where both these parts are strongly contracted at the same time. But it is only in the more unusual state of the respiratory organs, that this effect is produced; in ordinary cases it is probable, that the progressive motion of the chyle is effected solely by the muscular action of the vessels themselves. The thoracic duct soon after its commencement, lies under what are called the crura of the diaphragm, and it is supposed by Haller, that the passage of the chyle along the duct is materially promoted by the contraction of this part. (c) But  
it

(a) *Senac, Mem. Acad.* 1724.

(b) Note 29.

(c) *Haller, xxv. 2. 6,*

it is probable that in this, as in the former instance, it is only in the more violent exercise of the respiratory organs that any effect can be produced upon the motion of the chyle, and that in ordinary circumstances, the muscularity of the vessel is adequate to the propulsion of its contents. As a decisive argument in favour of this opinion, it has been observed, that the chyle has still continued its progressive motion along the thoracic duct after the thorax has been laid open, and consequently the effect of external pressure entirely removed. (a)

From the above remarks, we are led to conclude, that the effects produced by the dilatation and contraction of the thorax, which takes place in ordinary respiration, are much less considerable than was conceived by the older physiologists. In the experiments which they performed upon living animals, they neglected to make a due allowance for the unnatural situation in which the subjects were necessarily placed, and in those which they made upon the body after death, they proceeded upon the supposition, that the most complete change in the capacity of the thorax, of which it is capable, was in all instances

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stances

(a) *Senac, Mem. Acad. 1729. Cruickshanks on the Absorb. p. 169.*

stances produced. The experiments, therefore, exhibit the effects arising from a dilatation and contraction of the thorax, which can take place in the most extreme cases only, and the conclusions are consequently inapplicable to the phenomena of ordinary respiration.



## CHAP. II.

*The change produced by Respiration, in the Inspired Air.*

THAT a change is produced by respiration upon the air received into the lungs, could scarcely escape the notice of the most cursory observers of the operations of the animal œconomy. The ancients were not unacquainted with this circumstance, but the first accurate notions which we obtained respecting it, were deduced from the experiments of Boyle. This philosopher, who so successfully advanced the theory and practice of the various branches of experimental science, was peculiarly fortunate in his researches into the nature and properties of the atmosphere. He not only proved by means of the newly invented machine, the air-pump, the absolute necessity of air to the support of animal life, but he farther discovered, that the action of  
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the lungs is quickly suspended, unless they are furnished with a regular supply of fresh air. From this fact it was naturally concluded, that the air had undergone some important change during its continuance in the pulmonary vesicles, and a variety of hypotheses and conjectures were formed to account for this alteration. The knowledge which was then obtained respecting the air was, however, almost entirely confined to its mechanical properties, so that the theories of respiration formed during this period, were necessarily crude and imperfect. Boyle perceived that the air in passing through the lungs became loaded with a quantity of aqueous vapour, and he farther supposed, that it acquired what he calls recrementitious steams; (a) but respecting the nature of these steams he forms no conjecture. He observed also, that the air in which an animal had respired for some time, was considerably diminished in volume, an effect which he attributed to the loss of part of its elasticity or spring. The contemporaries of Boyle, for the most part, coincided with him, in his ideas respecting respiration; there were, however, some philosophers who supposed, that besides the addition of these vapours, the air, during its continuance in the lungs,

(a) *Boyle's Works*, vol. i. p. 99, & seq.; vol. iii, p. 371, & seq.

lungs, imparted something to the blood. Among these, the first in point of genius and originality was Mayow of Oxford. He investigated the properties of the air, and the effects produced upon it by respiration, with great acuteness, and concluded, that a peculiar volatile spirit, which was one of the constituents of the atmosphere, was absorbed by the blood during its passage through the lungs. Borelli, (*a*) Lower, (*b*) Willis, (*c*) and others, adopted opinions in many respects similar to that of Mayow; they imagined, that either a portion of the whole mass of air, or some particular constituent of it, was absorbed by the blood, and by this means converted this fluid from the venous to the arterial state. But so little real knowledge was at this time possessed respecting the composition of the atmosphere, that they entirely failed in their attempts to ascertain the nature of the matter absorbed, and their hypotheses appeared so extravagant, and so little founded upon truth, that their doctrines fell into discredit, became neglected, and at length were totally forgotten. (*d*)

Dr. Hales devoted much of his attention to this subject, and performed many experiments with

(*a*) *De Motu Anim.* p. 2a. prop. 113    (*b*) *De Corde*, p. 159—165.    (*c*) *Willis, pharm. Rat.* p. ii. p. 34.    (*d*) Note 30.

with a view to illustrate the manner in which the air is affected by the lungs; he concludes nearly as Boyle had done, that it acquires a noxious vapour, and that its elasticity is diminished. (a) The learned Boerhaave confesses his inability to explain the change which the air experiences by respiration. (b) The opinion of Haller was not materially different from that of Boyle and Hales; he has collected all the different theories which have been advanced upon this subject, and after reviewing them with his accustomed candour and perspicuity, he concludes that the air, when it is emitted from the pulmonary vesicles, is combined with a quantity of water, and a peculiar noxious vapour, and has its elasticity diminished. (c) Such was the imperfect state of our knowledge, when Haller wrote his *Elements of Physiology*! This noble monument of industry and genius was scarcely published, when Dr. Black commenced his experiments upon fixed air, and among other interesting discoveries, satisfactorily proved, that this peculiar gaseous substance is generated in the lungs during respiration. (d) Shortly after this period, the brilliant discoveries of Scheele and Priestley made

(a) *Statical Essays*, *passim*. (b) *Prælect*, t. v. p. 169. & seq. (c) *Notæ ad Boer. Prælect*. t. v. p. 170. *El. Phys.* viii. 3. 11; viii. 5, 19, 20. (d) Note 31.

us acquainted with the nature and composition of the atmosphere, and by this means effected a revolution in chemistry, and in all the branches of natural philosophy connected with this science. The atmosphere, which until this period had been regarded as an homogeneous, elementary body, was discovered by these celebrated experimenters, to be compounded of two aeriform fluids, possessing distinct properties, and serving totally different purposes in the œconomy of nature. These substances, which have since obtained the names of the oxygenous and the azotic gasses, were found to exist in the atmosphere, in the constant proportion of about 22 to 78, (a) but after an animal had breathed for any length of time in the same quantity of air, the oxygenous part appeared to be considerably diminished. Dr. Priestley repeated these experiments under a variety of circumstances, and imagined, that in these cases the air had undergone the same change in its properties, which it experiences from combustion, fermentation, the calcination of metals, or other operations by which the air is deprived of part of its oxygene, which, in consequence of his peculiar theory, he styles phlogistic processes. The change

(a) Note 32.



change produced in the air by passing through the lungs, according to this philosopher, consisted in the removal of part of the oxygene, and in the addition of phlogiston, and a quantity of aqueous vapour. (a)

About a year after the publication of Dr. Priestley's experiments, a memoir on respiration appeared from the pen of the celebrated M. Lavoisier. (b) After paying a tribute of respect to the genius of Dr. Priestley, he proceeds to an accurate examination of his experiments, and the conclusions which were deduced from them. He agrees with the Doctor in supposing, that the proportion of oxygene is diminished in air which has been respired, but upon a careful analysis of the residue, he finds it to differ from the air left after the calcination of metals, which is merely azotic gas, in containing a quantity of carbonic acid. He also observed, that the bulk of the air was somewhat diminished, and we learn in general from these experiments, that the changes produced in air by respiration, consist in the removal of part of the oxygene, in the addition of a quantity of carbonic acid gas, and in the diminution of its volume. He supposes that  
the

(a) *Phil. Trans.* 1776; *Observ. on Air*, v. iii. b. 9.

(b) *Mem. Acad.* 1777.

the azote is not affected by the process, and that it serves merely to dilute the oxygenous part of the atmosphere. (a)

In this paper M. Lavoisier does not mention the aqueous vapour which is so evidently discharged from the lungs by respiration; it is impossible that it could have been overlooked by so accurate an observer; we may therefore conjecture, that he omitted to mention it, because at this period he considered it as only diffused through the air expired from the lungs, by the process of evaporation, and not formed in consequence of the operation of any chemical affinities.

The conclusions of this philosopher respecting the changes produced by respiration upon the air taken into the lungs, are for the most part acquiesced in by the modern physiologists, and the researches which have been since made upon this subject are principally directed, either to ascertain with more precision the proportion of the respective ingredients in the air of expiration, or to frame hypotheses to account for the operation of the lungs in effecting these changes.

I shall

I shall defer my remarks upon the theory of respiration, until I have considered the alteration which is effected in the blood, by its passage through the lungs; in the remainder of this chapter I shall examine the experiments that have been made with a view to ascertain with more precision the amount of the changes produced upon the air.

When an animal has expired in a quantity of atmospheric air, in consequence of its being no longer fit for respiration, it is found that the whole of the oxygene is not removed from it, (a) and it appears, that independently of their bulk, different animals possess this power of abstracting oxygene in a greater or less degree. It is not easy to deduce any general conclusions from experiments of this kind, as there are several incidental circumstances, by which the death of the animal appears to be accelerated or retarded, but upon the whole, we are warranted to conclude from them, that the animals whose temperature is the highest, are the least able to exist in air wanting its due proportion of oxygene. Accordingly birds, who possess a temperature of about  $104^{\circ}$ , have been found to expire when two-thirds of the oxygene

(a) Note 34.

oxygen is removed ; a guinea-pig and a mouse, whose temperature is about  $98^{\circ}$ , can consume about three-fourths, while frogs are capable of living until nearly the whole of the oxygen is consumed. (a) It must however be observed, that when animals die in consequence of the want of fresh air, their death is not so much to be attributed to the deficiency of the oxygen, as to the presence of the carbonic acid, which when it exists in any considerable quantity, exercises upon the body a highly noxious influence. It is therefore necessary to guard against its effects by presenting to the air a substance which has the property of absorbing it as rapidly as it is produced. When these precautions were employed, Lavoisier found, that a guinea-pig could live without inconvenience in air which contained only one-fifteenth part of its bulk of oxygen, and when the proportion was still farther diminished, the only apparent effect produced, was a degree of drowsiness in the animal. (b) As the temperature of a guinea-pig is nearly that of a man, it is probable that human life might be supported with air of the same composition.

Another

(a) *Higgins's Minutes of a Society, &c.* p. 158. *Chaptal's Chemistry*, v. i. p. 127.

(b) *Mem. Acad.* 1789. p. 574.

Another question respecting the consumption of oxygene, of a more interesting nature, but of more difficult investigation, is the absolute quantity of this gas consumed by respiration in a given time. The first calculations which were made upon this subject, in consequence of the imperfect nature of the apparatus employed, and of the want of a sufficient dexterity in the management of pneumatic experiments, were unavoidably vague and inaccurate. The difficulty was much encreased by a circumstance first noticed by Dr. Crawford, and afterwards more fully investigated by M. Jurine of Geneva, and M. Lavoisier, that the respiration of the same animal in different states of the system, and under the operation of different external circumstances, affects the air in very different degrees. This curious fact, which affords an insight into some of the most important operations of the animal œconomy, must unavoidably produce great differences in the results of the best conducted experiments, and will render it impossible for us to arrive at more than an approximation to the truth. The circumstances which have been discovered to influence the chemical effects of the respiration are, the temperature of the air respired, the degree of muscular exertion, the state of the digestive organs, and the condition of the system as affected by fever; it is highly probable



probable that other circumstances will be discovered, by multiplying and varying our experiments upon the living body. These different affections of respiration will undergo a farther examination in the third part of this essay; they are noticed in this place in order to shew, that the greatest degree of certainty which we can attain upon this subject, is no more than an average deduced from a number of experiments, performed upon the body under the various circumstances in which it is placed, and the consequent changes to which it is liable.

An experiment performed by Lavoisier, upon a guinea-pig, seems to have been the first in which a perfect apparatus, and the necessary degree of accuracy were employed.<sup>(a)</sup> The animal was confined over mercury, in a jar containing 248 cubic inches of gas, consisting principally of oxygene. In an hour and a quarter, the animal breathed with much difficulty, and being removed from the apparatus, the state of the air was examined. Its bulk was found to be diminished by eight cubic inches, and of the remaining 240 inches, 40 were absorbed by caustic potash, and consequently consisted of carbonic acid gas. Taking 100 parts of this air, these numbers will

<sup>(a)</sup> *Mem. Acad.* 1780. p. 401—8.

will be as follows; the air was diminished to 96·5, or by 3·5 cubic inches, and of the remainder 16·5 were converted into carbonic acid gas, and absorbed by pot-ash, which reduces the quantity of air to 80 cubic inches. Towards the conclusion of the experiment, the air would be necessarily much less fit for performing the functions of the lungs than the air of the atmosphere, in consequence of the carbonic acid gas which it contained; but as the air employed was originally much purer than the atmosphere, the author supposes, that the quantity of oxygene destroyed, was probably about the same which would have been consumed under the ordinary circumstances of respiration;

The same philosopher performed a second experiment upon the same species of animal, with still more accuracy, in which pure oxygene was employed. (a) This experiment continued during an hour and a half, and the animal being then removed from the jar, the air was analyzed as in the former case. 1728 cubic inches of air were found to be reduced to 1673, i. e. had suffered a diminution of 55 inches, caustic pot-ash absorbed about 229·5 inches, leaving a residue of pure oxygene.

These

(a) *Ann. de Chim.* t. v. p. 261 & seq.

These numbers, estimated as in the former case, will be nearly as follows; 100 inches were reduced to 96.82. or by 3.18 inches, the pot-ash absorbed about 19 inches, reducing the whole quantity of air to 77.82 parts. The quantity of carbonic acid was here somewhat greater than in the former experiment, which may be attributed to the air employed being pure oxygene, and to the process having been continued for a somewhat longer space of time than in the former instance. Upon the whole, the results correspond as nearly as can be expected, from the very delicate nature of the experiments.

Dr. Menzies first attempted to ascertain the quantity of oxygene consumed by a man in the course of a day. He found by experiment, that one-twentieth part of air, which had been once respired, is converted into carbonic acid gas; this he concludes must have been oxygene, as that part of the air alone is affected by respiration. He conceives that 720 cubic inches of air are respired in a minute, of which consequently 36 will be consumed. From these data he estimates, that in the space of 24 hours 51840 cubic inches, or 17625.6 grains of oxygene, are consumed and converted into carbonic acid gas. In this calculation several important particulars

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appear to have been overlooked, and accordingly it will be found to differ from the results of the more accurate experiments, which have been since performed by M. Lavoisier, and Mr. Davy.

The experiments which were made by M. Lavoisier, in conjunction with his friend M. Seguin, were conducted with every possible attention to accuracy, and with an apparatus more complete, than any which has ever been employed in physiological researches. An account of them is detailed in two papers in the memoirs of the Academy of Sciences for the years 1789 and 1790; but notwithstanding the peculiar advantages under which they were performed, their results will not be found in all instances to coincide. M. Seguin was himself the subject of the experiments; the authors begin by pointing out the different effects which are produced by the respiration under the different circumstances in which the body is placed, and they farther remark, that individuals may probably differ in the absolute quantity of oxygene which they consume in the same circumstances. Making a due allowance for these variations, they conclude, that the mean consumption of oxygene by a man during 24 hours, is somewhat more than 22 French cubic feet, or 46037.38 English cubic inches,

which is a quantity



a quantity of gas which will weigh 15661·66 grains troy. (a)

Lavoisier was still continuing to pursue his experiments on this subject, and had constructed a very expensive apparatus, for ascertaining with still more precision, the amount of the several changes produced by respiration, when this great philosopher fell a sacrifice to the fury of Robespierre, and received sentence of death. He had already performed a number of experiments with his new apparatus, and earnestly requested a respite of a few days, in order to prepare them for publication; but his request was not granted. M. De la Place, who pronounced his eulogy, has fortunately given us the most important results; they will be found to differ in some particulars from the former experiments, though with respect to the quantity of oxygene consumed, they nearly coincide; it is stated that a man in 24 hours consumes 15592·5 grains. (b)

The only experiments on this subject which have been performed since the death of Lavoisier, are those of Mr. Davy, which appear to have been executed with great accuracy. From a

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number

(a) Note 35.

(b) *Suppl. to Enc. Brit. v. ii, p. 594.*



number of trials made upon his own respiration, he found, that 100 cubic inches of atmospheric air, after having once passed through the lungs, had lost between 4 and 5 parts of oxygene, hence he calculates, that 31.6 cubic inches of oxygene are consumed in a minute; (a) this will give 45504 inches in 24 hours, a quantity which will weigh 15471.36 grains. (b) This estimate coincides nearly with that of M. Lavoisier, though it was obtained by a different process and by the use of a different apparatus; we may therefore conclude, that between 45, and 46,000 cubic inches, or about 15500 grains, 2lbs. 8oz. troy, is the average quantity of oxygene consumed by a man in 24 hours.

Having ascertained the proportion of oxygene which is consumed in respiration, it next remains to determine the quantity of carbonic acid gas, which is produced. It appears that Dr. Black first demonstrated its existence in air which was emitted from the lungs, and that M. Lavoisier, afterwards examined it with more accuracy, and found that the air in which an animal had expired, contained about  $\frac{1}{6}$  of its bulk of carbonic acid gas. (c) In the experiment which this philosopher

(a) *Davy's Researches*, p. 431—4.

(b) Note 36.

(c) *Mem. Acad.* 1777.

lofopher performed with a more perfect apparatus, upon a guinea-pig confined in oxygene, the carbonic acid amounted to nearly  $\frac{1}{3}$  of the bulk of the whole air employed, when the animal had been detained in the apparatus until the air was reduced into a state, no longer fit for respiration. (a) These experiments, however, only prove what proportion of carbonic acid gas, will render air incapable of supporting life, without acquainting us with the quantity of this gas, produced under the ordinary circumstances of respiration.

M. Jurine of Geneva, appears to have been the first who attempted to calculate the absolute quantity of carbonic acid formed by the respiration of man; he imagined that it constituted about  $\frac{1}{10}$  part of the air emitted from the lungs. (b) Dr. Menzies instituted a set of experiments to discover the absolute quantity generated in a given time; he infers from them, that  $\frac{1}{20}$  part of air which has been once respired, is carbonic acid, and estimates, that a man in 24 hours, sends out from the lungs 51840 cubic inches, or nearly 4lbs. troy; (c) but this estimate is probably over-rated.

The

(a) *Ann. de Chim.* t. v. p. 261, & seq. (b) *Enc. Meth. Medicine*, t. i. p. 494. (c) *Menzies*, p. 50.

The circumstances which have been already pointed out, as influencing the consumption of oxygene, have at least as powerful an effect upon the production of carbonic acid gas. Accordingly we shall find the calculations of the most accurate experimenters upon this subject so widely different from each other, that it seems scarcely possible to arrive at any tolerable degree of certainty.

M. M. Lavoisier and Seguin, in their first memoir of 1789, estimate the average quantity of carbonic acid gas, formed by a man in 24 hours, at  $17720 \cdot 89$  grains troy; in their subsequent memoir, published in the following year, this quantity is diminished to  $8450 \cdot 24$  grains; and in the eulogy of La Place it is stated, that Lavoisier in his last experiments, reduced it still lower to  $7550 \cdot 40$  grains. Mr. Davy on the contrary, whose experiments seem to have been performed with great exactness, though with a less complicated apparatus than that employed by the French chemists, supposes the carbonic acid formed in 24 hours to amount to  $17811 \cdot 38$  grains, (a) a quantity which is not very different from that first announced by Lavoisier. Between such opposing authorities, it is not easy to decide; upon the whole I feel inclined

(a) *Researches*, p. 434.

clined to acquiesce in the experiments of Mr. Davy, and to conclude, that the quantity of carbonic acid gas, generated by a man in 24 hours, amounts to about 17800 grains or somewhat above 3lbs. troy.

Another of the changes produced by respiration upon the air, is a diminution of its volume. This circumstance was noticed by the older physiologists, but in consequence of the manner in which their experiments were performed, it was very considerably over-rated; they attributed it to a diminution of the elasticity or spring of the air. (a) Lavoisier, in the first memoir which he published upon respiration, ascertained the degree of diminution with more accuracy, and stated, that air when rendered unfit to support life, was reduced  $\frac{1}{80}$  in bulk. (b) The experiments of Dr. Goodwyn, afforded the same result; (c) but Dr. Crawford, by some mistake, was led to conclude that, when the process was conducted with accuracy, there was no perceptible diminution. (d) In the account which Lavoisier gives of the first experiment upon the guinea-pig, he found the diminution to amount to  $\frac{1}{31}$  of the bulk of the air employed, (e) and

in

(a) Note 37. (b) *Mem. Acad.* 1777. (c) Goodwyn, p. 51. (d) *On Animal Heat.* p. 146. (e) *Mem. Acad.* 1780, p. 401.



in the second set of experiments, the diminution was found to be  $\frac{1}{3}$  part; (a) the greater absorption in these cases probably depending upon the greater purity of the air employed. It is somewhat remarkable, that in the experiments performed by Lavoisier, in conjunction with Seguin, upon the respiration of man, though in other respects so remarkable for their accuracy, there is no mention made of this circumstance, nor is it noticed by La Place, in his account of the experiments in which Lavoisier was engaged, immediately previous to his execution. In these instances we cannot determine, whether Lavoisier conceived that no diminution actually took place, or whether he only neglected to notice it; upon the whole, the latter appears the more probable supposition.

The general fact of the diminution of bulk in respired air, has been since confirmed by Mr. Davy, though the exact degree of absorption varied so much in his different experiments, that it is difficult from them to fix upon a quantity which may indicate the ordinary amount of this diminution. In the consideration of this question, as in the preceding one respecting the consumption of oxygene, and the production of carbonic

(a) *Ann. de Chimie*, t. v. p. 261.



carbonic acid, there are two distinct objects of inquiry. We may examine the degree of diminution produced in a given quantity of air, in which an animal has been confined, until it is no longer fit for supporting the respiration; and in the second place, we have to ascertain the amount of the diminution which takes place in air that has only once passed through the lungs, as is the case in the process of ordinary respiration. The first of these points only was examined by Lavoisier. Mr. Davy has made experiments upon both. In air which had only once passed through the lungs, he found the diminution in different trials to vary from  $\frac{1}{70}$  to  $\frac{1}{100}$ ; (a) when he received the same air repeatedly into the lungs, it was found to be diminished as much as  $\frac{1}{18}$  of its original bulk. (b) The former experiments however alone indicate the effects of natural respiration, and taking an average of their results, the amount of the diminution will be about  $\frac{1}{80}$  part of the whole air received into the lungs. I am not acquainted with any experiments to ascertain the diminution of air that has been once respired, upon the accuracy of which we can depend, excepting those of Mr. Davy.

Mr. Abernethy, in opposition to the generally received opinion, conceives that the quantity of  
air

(a) *Researches*, p. 431—3.

(b) *Do.* p. 435.

air emitted from the lungs is greater than that received by them. This singular hypothesis is principally founded upon the supposition, that the carbonic acid discharged by respiration, is not generated by the union of the oxygene received by the blood as it passes through the lungs, with the carbone contained in this fluid, but that it is merely an exhalation from the surface of the vesicles. He estimates that 100 cubic inches received by the lungs, when discharged from them are augmented to  $107\frac{1}{2}$  inches. The diminution in the bulk of a quantity of air in which an animal has been confined, he attributes to its absorption by vessels with which he thinks it probable, the surface of the lungs is furnished, similar to those of the skin. (a)

If we have found it difficult to ascertain with accuracy the amount of those changes produced by respiration which we have hitherto examined, we shall probably experience still more uncertainty in determining the quantity of aqueous vapour that is emitted from the lungs. This substance is from its nature less easy to collect and measure than a permanently elastic fluid, and accordingly the physiologists who have attempted to discover the

the quantity of it mixed with the air of expiration, have for the most part proceeded rather upon calculations derived from collateral circumstances, than from the direct result of experiment.

The exhalation of water from the lungs in respiration, was a circumstance which could not escape the most cursory observer ; accordingly we shall find that it was noticed by the older physiologists, and was indeed regarded by them as one of the principal purposes which is served by this function. (a) Dr. Hales performed many experiments for the purpose of ascertaining its quantity ; he contrived to pass the air which he expired through a flask filled with wood-ashes, which in consequence of the pot-ash contained in them, have the property of strongly attracting moisture. By observing the increase of weight which the ashes had acquired in a given time, he estimates that the water emitted from the lungs in 24 hours, will amount to 9792 grs., above 20 oz. (b) The nature of his process, however, did not admit of much accuracy. Dr. Menzies attempted to solve this problem by actually collecting in an allantoid fitted to the mouth, the water emitted from the lungs in a given time ; his estimate is much less than that of Hales ; he supposed that the quantity of  
water

(a) Note 38.

(b) *Statistical Essays*, v. ii. p. 322—4.

water exhaled in 24 hours would amount to no more than 6 oz. or 2880 grains. (a) Mr. Abernethy, by breathing into a glass vessel of a peculiar construction, collected in an hour 180 grs. of water, containing, as he supposed, a quantity of mucous matter. According to his estimate, the quantity emitted in 24 hours, would amount to exactly 9oz. or 4320 grains, but as the substance which he obtained was not pure water, there must be some deduction made from it on this account. We are not informed what proportion the water bore to the mucus dissolved in it. (b)

The difficulty of actually collecting and weighing the pulmonary exhalation, is probably the cause which induced Lavoisier in his experiments upon respiration, to ascertain its quantity by a calculation, founded upon the proportion between its constituent parts, compared with the composition of the other substances which are received into and discharged from the lungs. He first determined by direct experiment the quantity of oxygen consumed, and of carbonic acid produced; the composition of carbonic acid is known, and by comparing the oxygen which had disappeared with the quantity which would have been necessary to form the acid, he found that the oxygen consumed was more than sufficient

(a) *Menzies*, p. 54.

(b) *Essays*, p. 141.



sufficient to compose the carbonic acid which was actually produced. (a) He supposed that this superabundant quantity of oxygene was employed in the formation of water, by uniting in the lungs with a portion of hydrogen; he estimates the amount of the water by knowing what quantity of it a given weight of oxygene can produce. This method employed by Lavoisier must be confessed to be extremely ingenious, but at the same time, before we can depend upon its truth, we must be well assured that the several propositions on which it rests, are themselves well founded. There are, I confess, several reasons which strongly incline me to suspect their validity, they will be considered in the following chapter; at present I shall only state the conclusions which this distinguished philosopher has deduced from his calculations. (b)

Lavoisier first formed an estimate of the water supposed to be generated in the lungs, from the experiments which he made upon the respiration of a guinea-pig confined in pure oxygene. He ascertained, that the carbonic acid produced would have acquired for its formation, a quantity of oxygene less by 55 cubic inches, than had in fact disappeared; this oxygene, the author adds, must either

(a) Note 39.

(b) Note 40.



either have been absorbed, and have entered into some combination with the mass of blood, or have united with a quantity of hydrogen discharged from the lungs, together with the carbone, and thus have produced water; he conceives that the latter supposition is the most probable. (a) The reasons for this opinion are not detailed as fully as might be wished, but he appears to have been influenced by the experiments, in which he conceived, that more caloric is disengaged by the formation of a given quantity of carbonic acid in the lungs, than from the formation of the same quantity of carbonic acid by the combustion of charcoal in the calorimeter. This excess of caloric, he imagined was derived from the union of the hydrogenous and the oxygenous gases. (b) In the experiment with the guinea-pig, the carbonic acid amounted to 107 grs., the water was estimated at about 25 grs. Nearly the same train of reasoning was pursued in the elaborate experiments upon the respiration of man, to which I have already referred, performed by the united efforts of Lavoisier and Seguin. In the first of these memoirs, the quantity of water emitted from the lungs of a man in 24 hours is stated to be no more than 337.18 grs.; but in the 2d., where a more perfect apparatus was employed, the weight of water

(a) *Ann. de Chimie*, t. v. (b) *Mem. Acad.* 1789.

ter expired in 24 hours, is estimated at 11180·57 grs., nearly 2lbs. troy, a quantity very much larger than the estimate derived from the former experiments. In the experiments performed by Lavoisier, of which an account is given by M. La Place, the quantity of water was supposed to be still more considerable, even as much as 13704 grs. in the course of 24 hours. The proportion between the carbonic acid, and the water discharged by respiration, as deduced from these last experiments of Lavoisier's, differs very considerably from that in the experiment performed upon the guinea-pig, and no less remarkably from each other. In the memoir of 1789, the carbonic acid was 17720·89 grs. the water only 337·18 grs. or as 1000 to 19 nearly; in the memoir of 1790, the carbonic acid was 8450·24 grs. and the water 11188·57 grs. or as 1000 to 1323; in the eulogy of La Place, the carbonic acid is 7550·40 grs. and the water 13704, or in the proportion of 1000 to 1815 nearly; from such very discordant calculations it is impossible to draw any conclusions. Mr. Davy has not formed any estimate of the quantity of water exhaled in respiration.

M. Lavoisier in his earliest experiments upon respiration, concluded that the azote remained entirely passive; and was received into and emitted from

from the lungs without undergoing any change; (a) to this opinion he always adhered, and it has been adopted by almost all the modern physiologists. Dr. Priestley, indeed, in his experiments, thought that there was an absorption of azote as well as oxygene, (b) but as the apparatus which he employed was not susceptible of perfect accuracy, and as his experiments were performed in the infancy of pneumatic chemistry, this circumstance was generally attributed to some accidental cause. Dr. Priestley's supposition has, however, lately received a strong confirmation from Mr. Davy, who uniformly discovered a deficiency of a small quantity of azote in his experiments upon respiration; he estimates the volume of azotic gas absorbed to be 5.2 cubic inches in a minute, (c) a quantity which, in 24 hours, will amount to about 2246.4 grs. or 4.68 oz. From the peculiar accuracy with which Mr. Davy's experiments appear to have been performed, I feel strongly induced to acquiesce in his opinion, notwithstanding the great authorities in favour of the opposite doctrine. The expressions which Lavoisier employs, render it probable that he derived his notion respecting the inactivity of the azote rather from theory than from actual experiment, and the positive

(a) *Mem. Acad.* 1777. (b) *Priestley's Exp. on Air*, v. iii. p. 380. (c) *Davy's Researches*, p. 434.

fitive assertion of so distinguished a philosopher seems to have deterred succeeding experimenters from any farther investigation of the subject. (a)

An opinion, directly the reverse of Mr. Davy's, was advanced by M. Jurine; he imagines that azote is actually generated by respiration. (b) But an attentive review of his experiments will easily explain the cause by which he was led to form so singular a supposition. After taking a full inspiration, he received the air from his lungs into four different vessels, and he found, that the portion which was last expired, contained a greater quantity of azote than the others. He also found, that when the air had been retained for some time in the lungs, or when the same quantity of air had been several times respired, that it contained more azote, than when only taken once into the lungs and immediately discharged from them, as in ordinary respiration. But it is obvious, that in all these cases, the increased quantity of azote referred only to the greater proportion which it bore to the rest of the air, and not in any degree to its absolute volume. M. Jurine has, however, performed another experiment, which appears more direct, and had there been no source of inaccuracy, would

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certainly

(a) Note 41. (b) *Enc. Meth. Medecine*, t. i. p. 493—7.



certainly have been decisive. He respired a quantity of pure oxygene, and upon examining it after it had been emitted from the lungs, he found it to be mixed both with carbonic acid and azote. But in this case there is every reason to believe, that this azote, which M. Jurine detected in the air of expiration, was derived from the contents of his lungs before the experiment. As it does not appear that he used any precautions to empty them before he breathed the oxygene, we may conclude that they would contain somewhat more than 200 cubic inches of azote, (a) which would immediately mix with the air inspired, and consequently be expelled along with it. In Lavoisier's very accurate experiment upon the guinea-pig, where the quantity of air previously contained in the lungs of the animal bore only a small proportion to the volume of gas in which it was immersed, after the animal was removed from the apparatus, and the carbonic acid absorbed by caustic pot-ash, the remaining oxygene was nearly as pure, as at the commencement of the experiment. (b)

After having examined in succession the different changes which are produced upon the air by

(a) Note 42.

(b) *Ann. de Chim.* t. v. p. 262.



by respiration, I shall conclude this chapter by a summary view of these effects, so far as the present state of our knowledge will enable us to form any precise ideas respecting them.

1. A quantity of oxygen is consumed in respiration; in ordinary circumstances atmospheric air, which has been once respired, loses nearly  $\cdot 04$  of its bulk of oxygen; in 24 hours a man consumes a quantity which will weigh about 2lbs. 8 oz.; somewhat more than 26 cubic feet.

2. A quantity of carbonic acid is generated by respiration; its volume is less than that of the oxygen absorbed, nearly in the proportion of 37 to 45; the weight of carbonic acid formed in 24 hours is about 3lb.; a volume of about 22 cubic feet.

3. The whole volume of the air is diminished by respiration; the degree of diminution is not very accurately ascertained, but it may be estimated at about  $\frac{1}{80}$  of its bulk.

4. A quantity of aqueous vapour, the amount of which is still undetermined, is emitted from the lungs.

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5. It is probable that a small portion of azote is absorbed, upon an average about  $\frac{1}{165}$  part of the air respired, making in 24 hours about  $4\frac{1}{2}$  oz. or 4 cubic feet. (a)

6. From the ascertained proportion in which the oxygene and pure charcoal exist in carbonic acid, it appears that a greater quantity of oxygene is consumed, than is necessary for the formation of the carbonic acid which is produced. (b)

(a) Note 43. (b) Note 44.

## CHAP. III.

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*The change produced upon the blood by Respiration.*

THE extreme vascularity of the lungs, and the great proportion of blood which is always present in them, induced even the oldest anatomists to suppose, that some important change was effected in this fluid by respiration; and the discovery of Hervey, which showed that all the blood in the body passes through the lungs during the course of each complete circuit, strongly tended to confirm this opinion. Soon after the doctrine of the circulation became generally received, the distinction between the arterial and the venous blood, was clearly pointed out, and it was perceived that this change is produced in the capillaries of the lungs. A number of conjectures, as may be imagined, were formed both to explain

plain the nature of the change which takes place, and the manner in which it is effected.

A numerous and learned class of physiologists supposed that the alteration which the blood experiences in its passage through the lungs, is in a great measure mechanical. They conceived that the blood, while in the pulmonary vessels, experiences a continual and violent agitation, by means of which, its particles, before loosely mixed, and consisting of several heterogeneous substances, are comminuted and perfectly united together, so that the whole mass acquires a uniform and homogeneous consistence. (a) Baglivi supposed that the blood was rarefied, (b) and Helvetius, that it was condensed in the lungs; (c) Boerhaave imagined that the particles acquired that peculiar organization, which he thought essential to the existence of perfect blood. (d)

Another set of philosophers, in which we meet with the names of Hervey, (e) Boyle, (f) Hales and Haller, imagined that the blood, in its passage through the lungs, parted with some  
noxious

(a) Note 45. (b) *Opera*, p. 457. (c) *Mem. Acad.* 1718. (d) *Prælect. t. ii. p. 184; 232 & seq.* Note 46. (e) *Exerc. de Motu Cordis*, p. 232. (f) *Works*, v. i. p. 99. & seq.; v. iii. p. 371. & seq.

noxious or superfluous matter, and with a quantity of aqueous vapour, which being mixed with the air, was removed by expiration.

A 3d class of physiologists were of opinion, that the change from the venous to the arterial state, depended upon something which the air imparted to the blood, whence it acquired its vital properties, and became adapted to the performance of its appropriate functions. None of these opinions is altogether correct even in the outline, and when their respective advocates proceeded to detail them more at length, they degenerated into mere fanciful hypotheses. Lower had endeavoured to prove, independently of any theory, that the bright scarlet colour which the blood acquires in the lungs, was owing to the action of the air, and we are now so well assured of the fact, that it seems to us somewhat surprising, that his experiments should have produced so little conviction. In order to disprove a notion which prevailed in his time, that the red colour acquired by the blood in the lungs, depended upon an innate fire kept up in the heart, he inflated the lungs of a dog recently killed, and he found that the blood was reddened exactly in the same manner as during the life of the animal. Another favourite hypothesis prevalent



lent in his time to account for the change of colour which the blood experiences in the lungs, viz. that it depends upon the comminution of its particles, owing to the agitation and preffure which it undergoes in this part of the circulation, he endeavoured to overthrow, by shewing, that the blood in the lungs of a newly killed animal would be reddened in the usual manner, though they were kept continually dilated, and consequently almost in a state of rest. As a further proof that the change of colour depends upon the action of the air, he observes, that when the blood is received into a basin, the surface only of the crassamentum, which is exposed to the air, acquires the scarlet colour, while the other parts remain of the dark purple hue. (a) The experiments and reasonings of Lower produced little conviction; the mathematicians (b) still thought it more probable that the red colour which the blood acquires in the lungs, depends upon the compression and agitation which it experiences in this organ. Haller strongly opposed the doctrine of Lower, and conceived that the different shades of colour depend upon the greater or less proportion of red globules in the blood, or the degree of compression which they suffer,

(a) *De Cordè*, p. 159—165.  
*tationes*, p. 62—70.

(b) *Pitcairne, Dissertations*.

suffer, and explains the bright scarlet of the top of the clot, upon the supposition, that the red particles subside to the lower part, in consequence of their gravity, or are more compressed than those at the upper surface. (a) After a long interval, the opinion of Lower was revived by Sig. Cigna of Turin ; he endeavoured to prove, that the change of colour in the blood, from a purple to a scarlet, in all cases depends upon the action of the air, and his experiments are such as fairly to establish the fact. They seem, however, to have excited little attention, and Cigna himself, in a subsequent memoir, appears almost inclined to desert his former opinions. (b) It was at this period that Dr. Priestley commenced his inquiry into respiration ; he repeated and varied the experiments of Cigna, placed his conclusions beyond the reach of doubt, and disclosed a new and important series of facts, which served as the ground-work for all the information that has been since obtained. The mutual action of the blood and air, which had been hitherto admitted as a plausible supposition, was now decidedly proved ; Dr. Priestley introduced a piece of dark-coloured crassamentum into an inverted

(a) *Prælect. Boer. Notæ ad, t. ii. p. 214. El. Phys. vi. 3. 17. Note 47.* (b) *Priestley's Exper. on Air, v. iii. p. 358—360.*

inverted jar of atmospherical air, the blood, after some time, assumed the arterial color, and the air was found to have experienced the same change in its chemical properties as by respiration. He afterwards proceeded to examine the effects produced upon the crassamentum by the component parts of the atmosphere applied separately, and by the other aeriform fluids which had been recently discovered. The purple coagulum was found to be reddened by oxygenous gas, still more rapidly than by the air of the atmosphere: while in azote, in hydrogen, and in carbonic acid, the contrary effects were produced, so that pieces of bright scarlet crassamentum were reduced to the dull purple hue of venous blood. The conclusions from these experiments were highly interesting and important: they shewed that the alteration of color which the blood experiences in the lungs, depends upon the oxygenous part of the atmosphere; and reciprocally, that the change which the air undergoes when received into the lungs, depends upon the action of the blood in the pulmonary vessels. The change which is in these cases effected upon the air, Dr. Priestley supposed to be similar to that produced by combustion, which, in conformity with his peculiar theory, he imagined to consist in the addition of phlogiston. He therefore

fore concluded, that the air in the lungs acquired phlogiston from the venous blood, and that this abstraction of phlogiston constituted the principal difference between venous and arterial blood. Dr. Priestley carried the resemblance between his experiments and the actual state of the lungs still farther, by introducing a moistened bladder between the crassamentum and the air; he found that in this case, the same changes were produced as in the former instance, and thus obviated an objection which might have been made against his experiments, that the blood and the air are not actually in contact in the lungs, but are separated by the membrane of the vesicles and the coats of the arteries. He likewise found, that the action between the air and the blood, was not interrupted by the intervention of a stratum of serum, or milk, but that when water, and some other fluids which he tried, were interposed, the change could be no longer produced.(a)

The train of experiment which had been opened so successfully by Dr. Priestley, was pursued with no less assiduity by M. Lavoisier. This philosopher, by examining with more accuracy the state of the expired air, found that it had acquired

(a) *Priestley's Exp. on Air. v. iii. p. 362—374. Phil. Trans. 1776.*

acquired a great addition of carbonic acid ; he had already demonstrated, that this substance is composed of oxygen and carbon, and he accordingly concluded, that the air had acquired its carbon from the venous blood. In his subsequent experiments he so far modified his original opinion, as to conjecture that, in addition to the carbon, a quantity of hydrogen was likewise disengaged from the venous blood, which uniting with another portion of oxygen, produced the water of expiration : according to Lavoisier, therefore, the chemical composition of venous and arterial blood differed in the former possessing a greater proportion of hydrogen and carbon.

That the air in the lungs had acquired from the venous blood a quantity of carbon, was a fact no longer to be doubted, but it was still uncertain in what manner this change of composition was effected. Either the oxygen of the air is combined directly with carbon abstracted from the blood, and the carbonic acid is generated by their union, or a quantity of oxygen is absorbed by the blood, and an equal quantity of ready formed carbonic acid discharged from it. Lavoisier in his first memoir proposes these two hypotheses without deciding in favour of either



either of them; but in his later papers he adopts the former, and consequently imagines, that all the changes produced upon the blood by respiration, are brought about in the lungs. The hypothesis of Lavoisier was adopted with some slight variations by most of his contemporaries; for the opinion of those chemists who still adhered to the doctrine of phlogiston, as Priestley and Crawford, must be regarded as scarcely differing from Lavoisier's, except in the name of the substance emitted from the venous blood, which was by one party called inflammable matter, or phlogiston, and was by the other, with more precision, stated to be carbone and hydrogen.

In order to complete this theory, it was necessary to point out an adequate source, whence the blood may be supplied with this carbonaceous or inflammable matter, which is perpetually discharged from the lungs. Dr. Crawford was the first who paid much attention to this circumstance; he proposed the following explanation. The solids of the living body have a constant tendency to decay; their particles are continually changing, those which are no longer fit for performing their functions are removed, and discharged from the body, while new ones are deposited in their room. The arterial blood which  
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is distributed to all parts of the body in the minute capillary vessels, is the vehicle by means of which this operation is performed; it conveys the nutritious matter to the different parts of the body, and deposits it in such a manner as to repair the necessary waste, while at the same time it receives the putrescent particles, which are become useless or noxious to the system, and carries them to the lungs, where they are united to oxygen, and discharged, together with the air of expiration. It is to the addition of this extraneous matter that Dr. Crawford attributed the change from arterial to venous blood, and by the removal of it in the lungs, the blood, he imagined, is brought back to the arterial state. (a) The hypothesis of Dr. Crawford, respecting the origin of the combustible matter, was generally received by the physiologists who adopted Lavoisier's method of explaining the changes produced by respiration. This philosopher himself does not indeed appear to have paid any particular attention to the subject; it may be inferred from some of his expressions, that at one period he inclined to the opinion of Dr. Crawford, but in his later memoirs he seems to consider the products of digestion as the immediate source of the

(a) *Observ. on Animal heat*, p. 150. & seq. Note 48.

the matter consumed in respiration. He however still supposes, that the changes are effected in the lungs, and it is therefore obvious that his theory must be imperfect, as the blood becomes completely venalized before it receives the contents of the thoracic duct.

This theory of respiration, though certainly simple and ingenious, was, however, found to be encumbered with many difficulties, for, independent of other objections, Dr. Crawford's explanation of the manner in which the blood acquires the supply of inflammable matter, is by no means satisfactory. It is indeed admitted, that the particles of which the solids are composed are perpetually changing, but it is contrary to all analogy to suppose, that the arteries are the instruments by which this change is effected. The body is provided with a distinct set of vessels, which from their office are called the absorbents, whose appropriate function it is to remove all superfluous matter. There can scarcely be a doubt, but that it is by the action of the absorbents, that the useless particles are removed, and these vessels have no communication with the sanguiferous system, except by the intervention of the thoracic duct, which receives all the substances absorbed, and pours them  
into

into the left subclavian vein. There is no passage hitherto discovered, by which the blood can receive any thing *ab extra*, from the time it leaves the heart, until it arrives at the mouth of the thoracic duct, when the circuit is nearly concluded, and the blood has already become completely venalized.

A second objection against this hypothesis, perhaps no less decisive, was derived from a circumstance which had been not unfrequently observed, that on some occasions the blood is converted to the venous state, while it still continues in the great trunks of the arteries. This change has been known to take place during surgical operations, in which the course of the blood along the arteries had been intentionally stopped, and also in cases where an extraneous, morbid body had pressed upon the vessels, and had prevented them from transmitting their contents in the usual manner. Where the tourniquet has been applied for any length of time to the trunk of a great artery, the blood which first flows through the vessel when the instrument is removed, is sometimes observed to be of the venous colour. Mr. Hunter remarks, that extravasated blood is in all cases of a dark purple colour, though there is every reason to suppose, that





their contents cannot be affected in the same manner by external agents. The capillaries of the lungs, are moreover exposed to the action of the oxygene of the inspired air, whereas the arteries in the other parts of the body, independent of the thickness of their coats, are not exposed to any external agent, which can be supposed to have the power of affecting the composition of the blood which they contain. It appears indeed, that the absence of oxygene is a more important circumstance than the mere thickness of the coats; as we learn from the experiments of Dr. Goodwyn, that the colour of the blood in the small veins of the neck of a rabbit, was in some measure brightened, by directing a stream of oxygene against the outer surface of the vessels, when the cellular substance was carefully removed. (a)

As it appeared therefore from these circumstances, that blood may be converted from the arterial into the venous state without the addition of any extraneous substance, it became necessary to abandon the theory of respiration which attributed this change to the absorption of inflammable matter, during the aortic circulation, and to form

(a) Goodwyn, p. 63.

form one in which the alteration might be produced by the mutual action of the constituent parts of the blood upon each other. (a) This was accordingly done by M. La Grange, who proposed a new hypothesis, which M. Haffenfratz adopted, and illustrated by several important observations and ingenious experiments. (b) The theory of M. La Grange, has been in some respects modified by Mr. Allen, (c) and with his improvements, appears to afford an easy explanation of every phenomenon.

According to this hypothesis, the blood in its passage through the capillaries of the lungs, absorbs oxygen, which is loosely united to the whole mass of fluid; by this union its colour is changed from a deep purple to a bright scarlet. The oxidated blood is then carried along the arteries; in the course of the circulation, the oxygen leaves the whole mass of blood, and forms an intimate union with a part of the hydrogen and carbon contained in it, by this operation it loses its bright colour and assumes the venous appearance. This portion of hydrogen and carbon, reduced to the state of an oxide, is then carried along the veins, until it arrives at

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(a) Note 49. (b) *Ann. de Chim.* t. ix.

(c) *De la Rive de Cal. Anim.* p. 35.

the lungs, where, after being united with an additional quantity of oxygene, it is discharged from the blood, and forms the carbonic acid, and the aqueous vapour which are found to exist in the air of expiration. According to this hypothesis, venous and arterial blood differ only in the arrangement of their component parts, in the latter case the oxygene is loosely combined with the whole mass, whereas in the former, it is in close union with a portion of hydrogen and carbone only. As a confirmation of the assumed principle upon which this hypothesis is founded, M. Haffenfratz adduces an experiment performed by M. Fourcroy, in which venous blood mixed with oxygene immediately acquired the scarlet colour, but after some time, though it was still exposed to its action, it again became purple, (a) owing, as he conjectured, to a more intimate union, which gradually took place between the oxygene and some part of the blood. Dr. Priestley had several years before observed, that arterial blood was rendered purple by exposure to azote, or to any of the gases which did not contain oxygene; in this case it may be inferred, that the gas attracted the oxygene from the mass of blood with which it was loosely combined, for if the  
oxygene

(a) *Ann. de Chim.* t. vii. p. 148.

oxygene had already been united to the carbone and hydrogene, so as to form carbonic acid and water, it is not probable that the mere exposure to azote, could have decomposed these substances.

The difference between the arterial and the venous blood, according to this hypothesis, being supposed to consist principally in the more or less intimate combination of the oxygene, M. Hassenfratz proposed to observe the effects which would be produced, by adding to the blood oxygene in a condensed state; he employed for this purpose the oxygenated muriatic acid. A quantity of it was accordingly poured into some blood, when the fluid acquired a deep and almost black colour. That this colour depended upon the oxygene, and not upon the muriatic acid, he proved by performing a second experiment, in which he used the common muriatic acid; the blood in this case was merely rendered brown. From the effects of the oxygenated acid, he inferred, that the oxygene, being presented to the blood in a condensed state, had immediately formed a union with the hydrogene and carbone, and had consequently produced the same change instantaneously, which in ordinary circumstances, is only brought about gradually during the course of the circulation.

circulation. M. Haffenfratz afterwards filled a number of tubes with arterial blood, and sealed them hermetically; after some time the blood became purple, and acquired the venous appearance. The same action appears to have taken place in this instance, between the constituent parts of the blood, which is produced in the capillaries of the aortic system, and it was effected in a situation where nothing could be absorbed by the blood or discharged from it. The oxygene which had been previously dissolved in the whole mass, and had imparted to it the bright scarlet colour, afterwards entered into a union with part of its hydrogen and carbone; the colour was changed to a deep purple, and from the situation in which the blood was placed, it had not the power of discharging the oxidated substances which it contained, or of attracting a fresh supply of oxygene.

These experiments certainly afford a strong confirmation of the truth of M. La Grange's hypothesis, and indeed they may be considered as demonstrating that a change in the blood, similar to that which it undergoes in the course of the aortic circulation, may be effected merely by a different arrangement of its constituent parts. Another circumstance in which this hypothesis possesses



possesses a most decided superiority over the former, though not noticed by La Grange and Hassenfratz, is the facility with which it accounts for the introduction into the blood of a regular supply of inflammable matter. According to the former theory, the change from the venous to the arterial state of the blood, depended upon the discharge of the superfluous inflammable matter, a process effected in the capillaries of the lungs; the blood was therefore supposed to return to the right side of the heart deprived of this substance, but in passing along the minute arterial branches, it acquired *ab extra* the hydro-carbonous matter, and was thus brought back to the venous state: I have already alluded the objections against this supposition. In the hypothesis proposed by M. La Grange, we may conceive the inflammable matter to enter the blood during any part of the circulation, as the different states which this fluid assumes, depend not upon the absolute quantity of the substances which it contains, but upon the state of their combination. The nature of the inflammable matter of the blood will be considered more fully hereafter, at present I shall merely state, that the obvious source of supply consists in the contents of the thoracic duct, which are poured into the left subclavian vein. (a)

M. M.

(a) *Skey de mat. combust. sang.*

M. M. La Grange and Hassenfratz, in conformity with the opinion of M. Lavoisier, that the oxygenous part of the atmosphere only was employed in respiration, supposed that this gas alone is absorbed in the lungs. The contrary opinion is, however, embraced by Mr. Davy. He observes, that from the experiments of Priestley and Cigna it appears, that venous blood can become florid through a stratum of serum, and that consequently either the whole of the air of the atmosphere, or the oxygenous part of it, must be dissolved by the serum before it can arrive at the red particles, so as to change their colour. He thinks it probable that the whole air is absorbed by the serum, and that in this condensed state, it is decomposed by the affinity which subsists between oxygene and the red globules. A small portion of the azote is also retained by the blood, but the greatest part of it is liberated without undergoing any change. (a)

Dr. Thomson also supposes, that the atmospheric air in its whole substance is absorbed, but that the greatest part of the azote is again discharged. In favour of this supposition, he urges the experiments of Dr. Priestley, and Mr. Davy,

(a) *Researches*, p. 447.

Davy, in which a small quantity of azote is consumed in respiration, and also the experiments of the latter upon the respiration of the nitrous oxide. As in this case the air of expiration consists of a part only of the gas inspired, mixed with a quantity of azote, it may be inferred, that the nitrous oxide is decomposed by the blood, a process which both Mr. Davy, and Dr. Thomson conceive, can only be brought about by its having been previously dissolved in this fluid. (a) But the state of combination in which the oxygene and azote exist in the air of the atmosphere, and in the nitrous oxide, differs so considerably, that even admitting the absorption of this latter compound by the blood, we shall not be authorized to infer, that the same action will take place with respect to the former. Upon the whole I think it probable, that the blood exercises its attraction upon the oxygene alone, but that in consequence of the powerful affinity which a large mass of one of these gases possesses for a small quantity of the other, the oxygene which is absorbed by the blood, still retains a minute portion of azote in combination. (b)

It appears then that the change produced in  
the

(a) *Davy's Researches, loc. cit. Thomson's Chemistry, p. iv. p. 492. Note 50.* (b) Note 51.

the composition of the blood, during its passage through the lungs is two-fold; a portion of inflammable matter is discharged from it, and a quantity of oxygene is absorbed. It seems upon the whole probable, that these changes are contemporary, and that the absorption of oxygene, and the discharge of inflammable matter, at least in ordinary cases, proceed in exact proportion to each other. The immediate effect upon the blood is to alter its colour from a deep purple to a bright scarlet. M. Haffenfratz's experiment, in which arterial blood assumed the purple hue, when enclosed in tubes hermetically sealed, (a) favours the supposition, that this change of colour is more influenced by the presence of the oxygene than of the inflammable matter.

As the blood is a heterogeneous fluid, composed of several substances, which are loosely connected together, and possess different chemical properties, it has been made a subject of inquiry, upon which of its constituents is the action of the air more immediately exercised. Of the two parts into which the blood separates by its spontaneous coagulation, the crassamentum and the serum, the former only has been made the subject of experiment,

(a) *Ann. de Chim.* t. ix. p. 269,

periment, and has been found to effect a diminution of oxygene, and a production of carbonic acid gas in the same manner, as when the entire mass of blood is exposed to the action of the air. From this circumstance, and also from the apparent similitude of the serum to other animal substances which are not known to have any specific action upon the air, it is inferred, that the crassamentum is the immediate agent in producing the change which is brought about in the process of respiration. The crassamentum itself is a compound body, consisting of fibrine and the red particles, the former of these substances appears to possess the same chemical properties with the muscular fibre, whereas the red particles are bodies of a peculiar organization, they give the blood its colour, and by the different shades which they assume, afford the principal indication of the approaches which it makes to the venous and arterial states. On this account it has been presumed, that the power of attracting the oxygene from the atmosphere, resides in the red particles. The nature, and even the form, of these particles is still involved in much obscurity; there is perhaps no subject in the whole range of physiology, respecting which more contradictory opinions have been advanced. (a) It has,

(a) Note 52.



has, however, been long known that they contain a quantity of iron, and from some late experiments performed by M. M. Fourcroy and Vaquelin, (a) the iron appears to be united to the phosphoric acid, the compound still retaining an excess of the metal in an oxidated state. The other component parts of the red globules have been examined merely by the vague method of destructive distillation, from which it can only be ascertained, that they contain the same chemical elements with animal substances in general. The experiments of M. M. Fourcroy and Vaquelin render it probable, that the colour of the red particles depends upon the phosphate of iron, but it is still uncertain, whether this metallic salt possesses the property of attracting the oxygen from the atmosphere, or whether this operation be not rather effected by the red particles in their whole substance. (b)

Lavoifier was the first philosopher, who formed any accurate idea respecting the nature of the inflammable matter discharged from the blood. In his first memoir in 1777, he considers it as composed entirely of carbone, but he afterwards conceived that a quantity of hydrogen was also disengaged from the blood, and this opinion has been

(a) *Système des con. chim.* t. ix. p. 152. (b) Note 53.

been adopted both by the French and English physiologists. The first of these effects, the discharge of carbone from the blood, appears to be sufficiently established; a quantity of carbonic acid is found in the air after expiration, which did not previously exist in it, while the air is found to be deprived of part of its oxygene. Carbonic acid is ascertained to be a compound of oxygene and carbone, and consequently the oxygene which has disappeared, is supposed to be converted into carbonic acid, by the addition of a quantity of carbone which has been abstracted from the blood. (a).

The proof of the discharge of hydrogene from the blood is less direct. It has been supposed, in the same manner with the carbone, to be emitted in combination with oxygene, and it is indeed true, that a quantity of water, the substance produced by the union of hydrogene and oxygene, is expelled along with the air of expiration. But we may point out another origin for the aqueous vapour without having recourse to the hypothesis of its generation, by the direct union of hydrogene and oxygene, either in the lungs, or in the course of the circulation. The inner surface of the bronchia and air vesicles, is lined with a copious

pious secretion of a mucous matter, and as the lungs are continually kept at a temperature of  $98^{\circ}$ , it may be suspected that the water exhaled from them proceeds merely from the evaporation of part of this fluid. As, however, the contrary opinion is generally received, and is countenanced by the highest authorities, it will be proper to examine upon what foundation it is supported.

It appears that the discharge of hydrogen from the lungs was first suspected by Lavoisier, in consequence of the experiment which he made upon the respiration of the guinea-pig in oxygen. (a) He was induced to form this conjecture by observing, that a greater quantity of oxygen had disappeared than what was sufficient to compose the carbonic acid which was produced, and he considered its union with hydrogen, and the consequent production of water, as the most probable method in which the superabundant oxygen could be employed. M. Lavoisier had already, in a former set of experiments, noticed, that somewhat more caloric was disengaged by the process of respiration than could be ascribed to the formation of the carbonic acid emitted from the lungs.

(a) *Ann. de Chim. t. v. p. 265; and t. xxi. p. 227.*  
*Mem. Soc. Roy. Med. 1782, 1783, p. 574.*

genous gas. The hydrogen, he supposes, is in this case absorbed by the blood, and he concludes, that if the absorption of hydrogen can convert arterial into venous blood, the discharge of this body must bring it back to the arterial state. (a) I think it a sufficient reply to this argument to remark, that the same change in the colour of arterial blood is brought about by the application of the azotic and carbonic acid gases, and even by placing it in vacuo. (b)

From this view of the arguments which have been adduced to prove the generation of water in the lungs, and at the same time from a consideration of the more obvious source of the aqueous exhalation, which is furnished by the moisture always existing on their surface, I do not hesitate to conclude, against the authority of Lavoisier, (c) that the discharge of hydrogen from the blood has been admitted without sufficient evidence, and that at present we have no proof of the emission of any substance from this fluid, depending upon the effects of respiration, except carbone. (d) I regard the water of the pulmonary exhalation as produced by evaporation from

(a) *Ann. de Chim. t. xxi. p. 225. & seq.* (b) *Priestley's exper. on Air, v. iii. p. 364.* (c) *Note 56.* (d) *Note 57.*

from the mucous fluid which lines the inner surface of the bronchia and vesicles, and which is separated from the blood by secretion; the water that is contained in the blood mixed with the serum, is poured into it along with the contents of the thoracic duct, which are at all times largely diluted with this fluid. (a)

The arterial has been observed to differ from the venous blood, in containing a less proportion of serum and water, and this difference has been attributed to the operation of the lungs; (b) but it may, with more propriety, be ascribed to the effects of secretion and transpiration. It is probable that the blood in its passage along the capillary arteries, is continually parting with its fibrine, in order to repair the waste which the muscular fibres experience, and the proportion of its serum will be consequently increased when it arrives at the veins. On the other hand, during its passage through the minute arteries of the lungs, a quantity of the water which it contained is employed in forming the mucus which lines the bronchia and vesicles, and in supporting the pulmonary transpiration. In the mean time the blood has received a fresh supply of chyle, which  
is

(a) Note 58.

(b) *Richerand*, p. 133.



is the immediate source whence the fibrine is produced.

The hypothesis advanced by M. Cuvier, (a) which supposes that respiration assists in the conversion of chyle into fibrine, appears to be better founded. We learn from accurate analysis, that there is a greater proportion of azote, and consequently a smaller proportion of carbone, in fibrine than in the chyle, the substance from which the fibrine is formed, so that in order to produce this change, it is necessary to remove from the chyle a part of its carbone; but this we have already found to be one of the principal effects produced by the process of respiration. (b)

The thoracic duct pours its contents into the left subclavian vein, at a little distance from the termination of the venous system in the right side of the heart. It has been observed, that when the blood arrives at this part of the circulation, the chyle still exists in a state of imperfect mixture with this fluid, but that after it has completed its passage through the lungs, the globules of chyle are no longer visible, and the whole forms one uniform mass. This intimate mixture

(a) *Leçons d'anat. comp. t. i. p. 92.*

(b) Note 59.

of the chyle with the other parts of the blood, has been considered as an effect of the respiration, but I think it more probable, that it is merely a mechanical operation, produced by the transmission of the blood through the minute network of vessels which composes the termination of the pulmonary veins, and the commencement of the corresponding arteries.

The discovery of Dr. Black, that bodies which indicate the same temperature, are frequently combined with different quantities of caloric; perhaps the most valuable of all the modern discoveries in chemistry, opened a path to the explanation of the singular phenomena of animal heat. This philosopher had previously examined the change effected in the air by respiration, and had found it to be in many respects similar to that produced by combustion; he accordingly inferred, that in the same manner as in the burning of an inflammable body, an extrication of caloric must take place in the lungs. This hypothesis respecting the origin of animal heat was, however, advanced by Dr. Black, only as a plausible speculation, and remained open to many objections, when Dr. Crawford turned his attention to the subject. He performed many elaborate experiments to determine the specific heat of  
a variety

a variety of different substances, and pursuing the analogy which had been pointed out by Dr. Black, between the effects of combustion and respiration, he particularly examined the state of the blood before and after it had passed through the lungs. The result of his labours was the discovery of the important fact, that the blood has its capacity for heat increased by its conversion from the venous to the arterial state, in consequence of the change produced in it by the action of the air. He determined the capacity of venous and arterial blood to be in the proportion of 10 to  $11\frac{1}{2}$ . (a)

As the production of animal heat appears to be one of the most important uses of respiration, I shall, in adherence to my proposed plan, defer to the 4th division of this essay, the farther consideration of this curious part of the animal constitution. I shall then examine at some length the theory of animal heat as originally proposed by Dr. Crawford, together with the various modifications which it has received from the ingenuity of those physiologists who have still more recently investigated the subject.

We have now reviewed in succession the changes

(a) *Observations on Animal heat*, p. 277.

changes which are supposed to be effected in the blood by respiration. It will be perceived, that notwithstanding the discovery of many important facts, the subject still remains involved in much obscurity. This must be, in a great measure, attributed to the difficulty of performing experiments upon a substance like the blood, composed of a number of different ingredients, connected together by a complicated system of affinities, which is liable to be disturbed by the operation of almost every external body. The difference between the arterial and the venous blood is rather inferred from the comparison of a number of observations, that have been made upon the body in its different states, than ascertained by the direct result of experiment, and consequently the information which we have acquired upon this subject, is still vague and imperfect. There are, however, a few points, which the labours of the modern physiologists seem to have clearly established, and from the progressive state of chemical science, we have reason to expect that their number will gradually increase.

The present state of our knowledge, respecting the change produced on the blood by respiration, may, I think, be comprized in the following propositions.

1. The

1. The blood which is expelled from the right ventricle of the heart, into the pulmonary artery, is of a dull purple colour; during its passage through the capillaries of the lungs, its colour is converted into a bright scarlet, in which state it is returned to the left auricle of the heart.

2. The change of colour is effected by the oxygenous part of the atmospheric air, which is received into the vesicles of the lungs.

3. The blood in passing through the lungs, emits a quantity of carbone, which is expired in combination with oxygene, under the form of carbonic acid gas.

4. The blood possesses an attraction for oxygene, and during its passage along the capillaries of the lungs, it absorbs a quantity of this substance.

5. To one of these circumstances, viz. the emission of carbone, and the absorption of oxygene, or to the combination of the two, is to be ascribed the change from the purple to the scarlet colour, which the blood undergoes during its passage through the lungs.

6. The



6. The same change of colour is produced, when a portion of the clot of venous blood, is exposed to the action of oxygenous gas, and the process is not interrupted by the interposition of a moistened membrane, or a stratum of serum, between the crassamentum and the air.

7. When arterial blood is exposed to the azotic, hydrogenous, or carbonic acid gases, or placed in the vacuum of an air pump, it assumes the purple hue of venous blood.

8. The oxygene, which is, in the first instance, loosely combined with the whole mass of blood, during the course of the circulation, gradually enters into a more intimate union with a part of its carbone, and forms with it an oxide. When this oxide is brought back to the lungs, it unites with an additional quantity of oxygene, so as to form carbonic acid, and is then removed with the air of expiration.

9. The blood receives its supply of carbonaceous matter from the contents of the thoracic duct, which are poured into the left subclavian vein.

10. It

10. It has been supposed that, besides the carbone, a quantity of hydrogene is discharged from the blood by the lungs, but, this opinion appears to have been adopted, without sufficient foundation.

11. In consequence of the abstraction of a quantity of carbone from the blood, the proportion of azote in the residuum is augmented ; it is also probable, that a small quantity of azote is absorbed by the blood, immediately from the atmosphere.

12. When blood is converted from the venous to the arterial state, its capacity for heat becomes increased ; the specific heats of venous and arterial blood are in the proportions of 10 to  $11\frac{1}{2}$ .

13. The blood, in consequence of its passage through the lungs, appears to have its constituent parts more uniformly mixed together, but this effect is to be attributed merely to the agitation and pressure which it experiences during its course through the minute and convoluted net-work of vessels which compose the capillaries of the lungs.

14. Besides

14. Besides the circumstances which have been enumerated, the arterial differs from the venous blood, in consequence of the substances separated from it to form the different secretions ; but these changes are not to be attributed to the effects of respiration, and it is absolutely impossible to appreciate their amount.

15. A considerable quantity of aqueous vapour is emitted from the blood, but this is the effect of transpiration, and consists of water which had been poured into the blood ready formed by the thoracic duct, along with the other products of digestion and absorption.

## CHAP. IV.

*On the respiration of the different Gases.*

IT is probable that no aeriform fluid, except the compound of oxygene and azote which exists in the atmosphere, is adapted to the permanent support of life. Of the other gases, there are some, which on account of their irritating nature, it is absolutely impossible to receive into the trachea; these properly constitute the non-respirable gases. There are, however, others which it is possible to inspire, though their employment is followed sooner or later by the extinction of life. I shall detail some of the principal experiments that have been performed on this subject, as the nature of the change produced upon the blood by common respiration may be in some measure illustrated, by observing the effects

fects which follow the use of the other gaseous bodies.

Soon after the discovery of the appropriate power which the oxygenous part of the atmosphere possesses, of supporting animal life, several trials were made of the effects which would result from breathing it in an unmixed state. The accounts given by those who were the subjects of these trials were various; Dr. Priestley, who first made the experiment, conceived that he felt an agreeable lightness in the chest,<sup>(a)</sup> some persons supposed that it produced exhilarating effects upon the system, while others imagined that the employment of this gas was followed by uneasiness and pain about the region of the thorax. These different sensations must be attributed in a great degree to the effects of imagination; in part, however, they may be ascribed to the gas which was employed in the earlier period of the pneumatic experiments having been often in an impure state, mixed with acid, acrid, or metallic particles.<sup>(b)</sup> A difference in the effects produced by the respiration of the gas, ought perhaps in some degree also, to be imputed to the manner in which it was received into the lungs, whether

(a) *Observ. on Air*, v. ii. p. 162.

(b) Note 60.



only in small quantities, or by deep and laborious inspirations, and whether it was employed in a condensed or a rarified state.

As Dr. Priestley was the first person who himself respired oxygenous gas, so he was likewise the first who observed the effects which it produced upon animals altogether immersed in it. His experiments were performed upon mice; they decidedly proved the superior power which this gas possesses of supporting animal life, but no other certain conclusions can be deduced from them.<sup>(a)</sup> M. Lavoisier afterwards turned his attention to this subject, and in the experiments upon guinea-pigs, to which I have already referred, he noticed with more accuracy the effects resulting from the respiration of oxygene. He examined the state of the internal organs of animals which had been for some time confined in this gas, and he conceived that a degree of redness and turgescence of the vessels was produced, and other effects which indicated that the sanguiferous system had been in a state of encreased action.<sup>(b)</sup> There is, however, reason to infer, that in this case, either the gas employed was in an impure state, or that there were some circumstances attending the situation

<sup>(a)</sup> Note 61.

<sup>(b)</sup> *Mem. Soc. Roy. Med.* 1782, 1783. p. 576.

tion of the animals, or the manner in which the experiment was conducted, which affected the results, for the same philosopher, in the subsequent memoir of 1789, where there appears to have been the greatest attention to accuracy, and where the most perfect apparatus was employed, forms entirely opposite conclusions. In this paper, we are informed, that he confined guinea-pigs in pure oxygene, and in mixtures of oxygene and azote, in different proportions, until the former constituted only one-fifteenth part of the compound. In all these cases, he found that the same quantity of oxygene was consumed, a circumstance, he observes, in which respiration differs remarkably from combustion, though in many respects, these operations are similar to each other. The effects produced upon the animals were precisely the same, whether they were confined in pure oxygene, or in any of the mixtures of it, except that when the proportion of azote was very large, they exhibited marks of drowsiness. The author expressly informs us, that neither the temperature nor the circulation were in any respects affected by the inspiration of pure oxygene for the space of several days.(a) These experiments must be considered as very valuable; there is no reason to doubt

(a) Note 62.

doubt their accuracy, and they may be relied on with more confidence, as the author seems to have had no peculiar theory in view when he performed them; indeed the results are different from what we might previously have expected, and are unfavourable to the analogy which Lavoisier had always endeavoured to establish between respiration and combustion.

We have an account given by Dr. Higgins of the respiration of pure oxygene by the human subject; in one experiment, thirty-eight pints of this gas were respired without interruption. No inconvenience was experienced, a sense of warmth was, however, produced in the chest, and the pulse was considerably quickened. (a) From the experiments of M. Lavoisier, I am induced to conjecture, that this increased rapidity of the circulation must be attributed rather to the greater efforts which almost necessarily attend respiration when continued for any length of time in the way in which it was performed in this process, than to the effects of the application of the increased quantity of oxygene to the lungs. Dr. Higgins, in a 2d experiment, breathed a quantity of oxygene under an additional pressure, and by

(a) *Minutes of a Society, &c.* p. 144—6.

by very full inspirations. He conceived that by these means its consumption was much promoted, (a) but more numerous and accurate experiments will be required before this inference can be fairly established.

In the year 1794, Dr. Beddoes published his experiments upon this subject. They were performed upon rabbits, and the attention was principally directed to an examination of the state of the internal organs of the animal, after it had been subjected for some time to the influence of pure oxygene. The author commences his inquiry, by stating, that "Dr. Priestley and "M. Lavoisier, found animals either to die or to "become exceedingly ill in such air," (oxygene little diluted) "while it continues more oxidated "than the atmosphere." (b) There is no reference made to any particular passage in the writings of these philosophers, but from the extracts which I have given, it would appear that their sentiments are altogether of an opposite nature. (c) Dr. Beddoes quotes the earlier experiments of M. Lavoisier, but the later and more decisive ones in the memoir of 1789 are not noticed. It is on the contrary taken for granted, that when an

(a) *Minutes of a Society, &c.* p. 152. (b) *Beddoes on fact. Airs*, pt. i. p. 13. (c) See Notes 61 and 62.

lungs, (a) and this difficulty he supposed might also be removed, by admitting the formation of a quantity of water, a process in which, as well as in the generation of carbonic acid, caloric is evolved. It is evident, that the first of these arguments for the formation of water in the process of respiration, depends, in a great measure, upon the truth of the peculiar theory advanced by M. Lavoisier, which supposes that the oxygene attracts the inflammable matter of the blood during its passage through the lungs, without entering into that fluid. But according to the hypothesis advanced by M M. La Grange and Hassenfratz, the whole of the oxygene employed in respiration is conceived to be in the first instance absorbed by the blood, and there is no reason to conclude, that the exact quantity of this gas, which is received into the system by the lungs, is always discharged by the same channel. Even if we were to admit this to be the case, it would still remain to be proved, that the carbone which enters into the composition of the carbonic acid, had previously existed in that state of combination which constitutes pure charcoal. (b) This substance was conceived by Lavoisier to be an elementary body, and his calculations were formed upon

(a) *Mem. Acad.* 1780, p. 405. 1789. p. 569. (b) Note



upon this supposition; but the experiments of Guyton and Tennant, have demonstrated, that what was formerly considered as pure carbone, already contains  $\cdot 36$  of oxygene, and that consequently, carbonic acid consists of a greater proportion of oxygene than Lavoisier supposed. It is however unnecessary to pursue this calculation, as we are not assured of the truth of the previous steps upon which the hypothesis rests.

With respect to the want of correspondence between the quantity of caloric evolved in the lungs, and the carbonic acid generated, although we might endeavour to remove this difficulty, by taking into account the caloric disengaged during the formation of water, provided this circumstance were decisively proved by other evidence, yet it must be confessed, that the experiments respecting animal heat, and the apparatus employed for measuring its exact quantity, are of too delicate a nature to serve as a proof of the fact.

In addition to these arguments, which were urged by M. Lavoisier, to prove the generation of water in respiration, M. Seguin adduces the experiments of Cigna, Priestley, and Hamilton, where the scarlet colour of the arterial blood was rendered purple by exposure to hydrogenous

an animal has been for some time confined in air, which contains a greater proportion of oxygene than the atmosphere, the body becomes, as he expresses it, oxygenated. He had previously formed an hypothesis respecting the operation of oxygene upon the system, according to which the long continued inspiration of it was supposed to induce an inflammatory state, and he accordingly found indications of this peculiar condition in every part which he examined. The lungs were florid, and in some places they were marked with livid spots, the pleura was inflamed, the heart retained its irritability for a longer space of time than usual, the blood coagulated more rapidly, and the liver was of a lighter colour than ordinary. The whole system was, as it were, so completely saturated with oxygene, that the immersion in hydrogenous gas, and in water, did not destroy the animals, until after a longer interval than usual; a fact which remarkably coincided with the theory respecting the cause of death in submersion, which the author had previously formed. The great difference between the results of these experiments, and those of M. Lavoisier, will appear the more remarkable when we consider, that in the experiments of the latter, the animals were exposed to the influence of the oxygene for several days, whereas those of Dr. Beddoes

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occupied a much shorter space of time. Indeed we are expressly informed, that the air in which the animal had been confined, "seemed to have suffered little diminution either in quantity or quality," (a) notwithstanding which, these singular changes were induced upon the body. It is impossible to reconcile opinions and experiments so contradictory as those which I have related; our judgment must in such instances be guided partly by the general character for correctness and impartiality which we attach to the works of the respective authors, and partly by comparing them with the results of experiments which in any degree resemble them.

We are indebted to Mr. Davy for some interesting experiments upon the respiration of oxygen, which appear to have been performed with his accustomed correctness. He supposes that life is finally destroyed by the long-continued employment of this gas, and refers for the proof of his opinion, not only to the results of his own experiments, but to those of Dr. Beddoes and M. Lavoisier. He does not, however, notice any particular passage in the works of this latter author, and considering the very direct nature, both of his

(a) *Beddoes on Airs*, pt. i. p. 16.

his experiments and opinions upon this point, it may be conjectured, that Mr. Davy has been inadvertently misled by the assertions of Dr. Beddoes. Indeed Mr. Davy's own experiments appear to lead to conclusions diametrically opposite to those of Dr. Beddoes. He respired the same portion of oxygene for several successive times, and then accurately ascertained the exact quantity of it which had been consumed, and of carbonic acid which had been generated, when he found, that both these quantities were less than in the ordinary respiration of atmospheric air, he obtained the same results when mice were made the subjects of his experiments. (a) It appears therefore, that in this instance, if any alteration be produced in the system, it must arise rather from a deficiency than from an excess of oxygene. But notwithstanding the attention with which these experiments appear to have been conducted, an objection may, I think, be urged against them from the erroneous estimate which Mr. Davy formed of the capacity of the thorax in a state of complete expiration, a datum which is employed in the calculation of the quantity of oxygene left after the process. (b)

From the review of the experiments which

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(a) *Researches*, p. 444.

(b) Note 62.



have been made upon the subject, I think myself justified in concluding, that we have not yet any direct proof that the respiration of oxygene is injurious to the animal œconomy. It is, however, not impossible that it may be the case, were the gas applied to the lungs for a sufficient length of time, but we have no ground either from experiment or observation, to enable us to determine what peculiar morbid action would be induced.

Among the remaining respirable gases, that which appears to be the least injurious to the living body is the oxide of azote. This aeriform fluid was first discovered by Dr. Priestley, and was by him supposed to be "in the highest degree noxious to animals." (a) The society of Dutch chemists, who afterwards examined its properties with more accuracy, coincided with Dr. Priestley, as to its effects upon animal life. (b) The experiment was, however, repeated by Mr. Davy, and he discovered not only that this gas may be respired for a short time without inconvenience, but that the employment of it is succeeded by a singular excitement of the nervous system, which differs from that produced by alcohol.

(a) *Observ. on Air*, v. ii. p. 55. (b) *Journ. de Phys.*  
t. xliii. p. 329.



hol and opium, in not inducing a subsequent state of exhaustion. Mr. Davy has also proved, that it is absorbed by the blood in considerable quantity, and he imagines that it is decomposed by this fluid. (a) I have already ventured to express my doubts as to the correctness of this latter conclusion. (b)

Hydrogenous gas has been frequently respired, (c) and it is now pretty generally agreed, that it is altogether passive when received into the lungs, and that death succeeds the employment of it in consequence of the exclusion of oxygene, in the same manner as by suffocation, or drowning. M. Lavoisier distinctly ascertained this fact, in his experiments related in the memoir of 1789, and it has been since confirmed by Mr. Davy. (d) It must be remarked, however, that a contrary opinion respecting the effects produced by the respiration of hydrogen has been maintained by some eminent chemists, even by Dr. Priestley himself; (e) but his experiments were made in the earlier period of the pneumatic chemistry,

(a) *Researches*, p. 409, & seq. (b) See Note 50.

(c) *Scheele on Air and Fire*, p. 160. *Fontana, Phil. Trans.* 1779, & *Journal de Phys.* t. xv. *Pilatre de Rozier, Journ. de Phys.* t. xxviii. p. 425. (d) *Researches*, p. 466.

(e) *Observ. on Air*, v. i. p. 229.

chemistry, when the gases were frequently employed in an impure state, and the experiments of Mr. Davy, clearly demonstrate that hydrogenous gas produces different effects upon the system, according to the substances from which it is procured. (a)

The experiments that have been made upon the subject of azotic gas are few and imperfect; it has been generally supposed, that it exercises no noxious effects upon the blood, but like hydrogen, destroys life simply by preventing the access of oxygen. Dr. Higgins indeed remarks, that an animal dies sooner when immersed in this gas, than from the simple interruption to respiration, (b) but we are not informed upon what data this opinion is founded. Mr. Davy likewise experienced the sense of suffocation more speedily from the use of azote, than from that of hydrogen, but it appears that the gas employed in the experiment contained a quantity of carbonic acid, (c) to which we may, with great probability, ascribe its noxious effects; and the same philosopher, when speaking in general terms of the action of azote in respiration, seems to consider it as merely excluding oxygen. (d) This opinion

(a) *Researches*, loc. cit. (b) *Minutes of a Society*, &c. p. 133. (c) *Researches*, p. 466. (d) *Do.* p. 334.

opinion is adopted by Dr. Thomson, *(a)* and was uniformly maintained by M. Lavoisier. *(b)* It would certainly appear reasonable to conclude a priori, that a substance which enters so largely into the composition of the atmosphere, and which consequently composes so great a proportion of the contents of the lungs, could not exercise any noxious effects upon the animal system.

The only remaining gas which is capable of being received into the lungs, is the carbonated hydrogen or hydrocarbure. If it be inspired in an undiluted state, it is followed by instant death, and when employed in small quantity only, mixed with atmospheric air or with oxygen, if it be used for any length of time, it induces vertigo, dimness of sight, convulsions, loss of sensation, and in short, every symptom of approaching dissolution. It evidently acts more rapidly and powerfully than those gases which merely exclude oxygen from the blood, and must consequently be considered as exercising a positively noxious influence upon the animal œconomy. These fatal effects have been attributed by some authors to its suddenly abstracting all the loose oxygen from the

*(a)* *Chemistry*, v. iv. p. 492.    *(b)* *Mem. Acad.* 1789. p. 574. *Et alibi.*

the blood, and thus completely depriving that fluid of the property by which it supports the irritability of the muscular fibres of the heart and arteries. To this hypothesis it may be objected, that the hydro-carbonous gas does not in other cases appear to possess that powerful attraction for oxygene, which could induce so rapid a change in the composition of the blood; and it may be farther urged, that if it be taken into the lungs, mixed with a quantity of oxygenous gas, provided it be not too much diluted, it will still be found to produce its accustomed fatal effects. The noxious influence of this gas is, I conceive, with more propriety referable to its action upon the nervous system, upon which it produces *directly sedative* effects. (a)

All the remaining gases are found to be strictly non-respirable, i. e. incapable of being admitted into the trachea. It is obvious that this must be the case with the irritating acid or alkaline gases, and with the nitrous gas, which during its passage into the lungs, must unavoidably be brought into contact with oxygene, and thus produce nitrous acid vapour. The only substance respecting the respirability of which there could be any doubt, is the

the carbonic acid gas, which, though possessed of the decided characteristics of an acid, exhibits them in a much flighter degree than that in which they generally exist. The impossibility of taking it into the lungs, even by the most powerful voluntary efforts, when it composes a large proportion of the air, was however proved by the experiments of M. Pilatre de Rozier,<sup>(a)</sup> executed with that intrepidity which formed so remarkable a trait in the character of this philosopher.

With these remarks I shall conclude the second division of my subject, and at the same time, the present volume. The third part of the essay, in which I propose to treat of the different affections of respiration, must principally consist of original observations and experiments, and though I have made some progress in them, they are still in too imperfect a state to meet the public eye. Physiologists have in general been more inclined to form hypotheses than, to execute experiments, and it has necessarily ensued from this unfortunate propensity, that their science has advanced more slowly than almost any other department of natural philosophy. The subject of respiration has indeed of late attracted the attention of the chemists,

(a) *Journ. de Phys.* t. xxviii. p. 422. & seq.



chemists, and its phenomena have been investigated with considerable assiduity. A proportional advancement in our knowledge has been consequently produced, but there still remain many questions to be solved, and many difficulties to be removed. Some of these difficulties have appeared in those parts of the subject which have passed under our review, but they will be found to exist in much greater number in the remaining divisions. How far it will be in my power to remove any of them, it would at present be presumptuous in me to determine, though I think myself justified in auguring favourably of the events of my experiments, from the interesting results of those which I have already performed.

NOTES.

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## NOTES.

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### Note I.

M. CUVIER states, that there are some very imperfect animals, in which no appearance of respiratory organs has yet been discovered ; (a) but we are induced by analogy to suspect, that in these cases, future and more accurate observations will enable us to detect the apparatus by which a function is performed, which seems absolutely essential to the existence of animal life.

In conformity with the custom of the modern physiologists, the word respiration is here employed to signify that operation by which the blood and the air are enabled to act upon each other,

(a) *Leçons d'Anatomie Comparée*, t. i. p. 42.

other, though, according to the etymology of the term, it ought only to be applied to those animals that are furnished with lungs. The necessity of oxygene for the support of the life of fish, was first discovered by Dr. Priestley (*a*); it has been since confirmed by Sig. Carradori (*b*) and others.

### Note II.

A good description of the comparative anatomy of the organs of respiration may be found in Mr. John Bell's Anatomy, *vol. ii. c. 3.*

### Note III.

It has been imagined by some physiologists, that insects differed from other animals in the chemical change which their respiration produced in the air, but this opinion is proved to be erroneous by the experiments of Vauquelin. (*c*)

### Note IV.

This idea respecting the termination of the  
bronchia,

(*a*) *Experiments on Air*, v. iii. p. 382—7.      (*b*) *Ann. de Chimie*, t. xxix. p. 171, 2.      (*c*) *Ann. de Chim.*  
t. xii. p. 273.

bronchia, which is generally adopted by the modern anatomists, rests altogether, or in a great measure, upon the microscopical observations of Malpighi. Succeeding physiologists have not been able to trace this minute structure, and some authors of respectability have not scrupled to deny its existence. Among others, Helvetius endeavours to prove that the bronchia terminate in a cellular or spongy tissue, composed of a delicate or membranous substance, the cells of which have no determinate figure, or regular connexion with each other. (a) There is indeed every reason to suppose with Cheselden, (b) that Willis and other subsequent anatomists, who have described and delineated distinct, rounded vesicles, have exceeded the limits of accurate observation. Malpighi, upon whose authority the belief of the existence of this peculiar structure is founded, expresses himself in the following manner, “ Dili-  
 “ genti indagine inveni totam pulmonum molem,  
 “ quæ vasis excurrentibus appenditur, esse aggre-  
 “ gatum quid ex levissimis & tenuissimis mem-  
 “ branis, quæ extensæ & sinuatae pene infinitas  
 “ vesiculas orbiculares, & sinuosas efformant, ve-  
 “ luti in apum favis alveolis ab extensa cera in  
 “ parietes conspiciamus.” And again, “ mem-  
 “ branæ istæ vesiculæ videntur efformari ex de-  
 “ finentia

(a) *Mem. Acad.* 1718.(b) *Cheselden's Anatomy*, p. 173.

“ finentia tracheæ, quæ extremitate, et lateribus  
 “ in ampullosos sinus faceffens, ab his in spatia,  
 “ & vesiculas inæquales terminatur.”(a) This  
 description by no means conveys the idea of the  
 regularly rounded vesicle, which has been adopted  
 by succeeding anatomists, and is delineated by  
 Willis.(b) The analogy of the frog and tortoise  
 is certainly in favour of the opinion of Helvetius;  
 the structure of their lungs, which is less minute,  
 evidently discovers the termination of the bron-  
 chia to be a congeries of irregular, membranous  
 cavities. Haller, with his accustomed candour,  
 after stating the opposite authorities, confesses  
 that he was at one time inclined to doubt the  
 generally received opinion, although he after-  
 wards saw reason to acquiesce in the doctrine of  
 Malpighi.(c) M. Dumas, who gives a full ac-  
 count of the different opinions that have been  
 entertained upon this subject, seems inclined to  
 adopt an intermediate one between that of Hel-  
 vetius and Haller.(d)

#### Note V.

Many physiologists, sanctioned by the autho-  
 rity

(a) *Epist. de pulm.* i. (b) *Pharm. Rat. sect. i. cap.*  
*i. ; Tab. 3.* (c) *El. Phys.* viii. 2. 29, 30. & *Notæ ad*  
*Boer. Prælect. t. ii. p. 154, 5.* (d) *El. Phys. t. iii. p.*  
 435—9.



rity of Galen and Harvey, have supposed, that  
 there was a quantity of air contained between  
 the lungs and the thorax; Hoadley (a) and  
 Hales (b) endeavoured to prove this point by ex-  
 periment. Indeed so prevalent was this opinion,  
 even half a century ago, that Haller thought it  
 necessary to apologize for embracing the opposite  
 side of the question; his philosophic language on  
 the occasion deserves to be quoted. “Candide,  
 “nulla parte sui roboris diffimulata, adversariorum  
 “argumenta proposui; superat, ut ostendam,  
 “cur ab iis aut auctoritatibus, aut experimentis  
 “non passus sim me persuaderi. Neque enim  
 “hic quid-quam valuit aut præceptoris mei gra-  
 “tia, aut adfinis mei, adque eam usque primam  
 “illam Boerhaavianæ sententiæ propugnationem  
 “fatis amici, Hamburgeri odium, neque concer-  
 “tationis inde natæ tædium; neque ulla certe  
 “causa mei dissentendi est, præter ea, quæ  
 “nunc produco, rationum momenta. Neque me  
 “malus pudor retinuerit. Facile enim video si-  
 “quidem contrariam meæ opinionem expensam  
 “præponderare sentirem, veram me gratiam,  
 “sinceramque laudem, apud candidos virtutis  
 “æstimatores meriturum, si ex longo opere,  
 “quod in ætatem durare velim, errorem etiam  
 “fero

(a) *Lectures on Respiration*, chap. i. (b) *Statical  
 Essays*, v. ii. p. 81.

“fero adgnitum proscriberem.” (a) Many anatomists and physiologists of the first respectability conceived that they had detected small pores in the vesicles of the lungs, which permitted a part of the air in them to transude into the cavity of the thorax, between the pleuræ; (b) but later observations have decided against the existence of these passages.

#### Note VI.

The opinions of the most learned amongst the ancients respecting the nature and use of the diaphragm, are very remote from the ideas which we now entertain upon this subject. Fabricius seems to have been the first who clearly pointed out its muscularity, and its great importance in the act of respiration. (c) Some anatomists, among others Senac (d) and Winslow, (e) from the connexion of the centre of the diaphragm with the parietes of the thorax and the mediastinum, were led to suppose, that this part remained immovable, and that it was the fibres of it only which had their position changed by the contrac-

tion

(a) Haller, viii. 2. 5. Dumas, t. iii. p. 423. & seq. (b) Boerhaave *Prælect. t. v. pars. i. p. 22.* (c) Fabricii *Op. De Resp. c. 8. &c.* (d) *Mem. Acad. 1724, & 1729.* (e) *Mem. Acad. 1738.*

tion of its muscular fibres; but Haller, and most of the modern anatomists conceive that the whole of the diaphragm is lowered during inspiration, (a) although it is generally admitted, that the degree of motion in the centre is less considerable.

### Note VII.

The contests of anatomists on this subject have been at least as violent, as respecting the existence of air in the cavity of the thorax; (b) Haller prefaces his account of the controversy with the following remark: "Multo plus de  
"istorum musculorum officio dubitationis ab  
"omni tempore fuit, cumque acres inimicitias  
"harum carnum actio mihi conciverit, accuratius erit in hac quæstione versandum: at interim liceat, inter miseras vitæ humanæ etiam  
"istam deplorasse, potuisse ob res adeo ab omni  
"utilitate propria, atque conservatione nostri  
"remotas, tantas iras, tantasque & tam acerbis nobis intentare lites." (c) The older anatomists generally supposed from the situation and structure of the intercostals, that the externals by their contraction increase the capacity of the chest, and consequently serve for inspiration, but that the internals on the contrary diminish the  
M cavity

(a) Haller, viii. 1. 36.

(b) Dumas, t. iii. p.

492-6. (c) Haller, viii. 1. 13.

cavity of the thorax. This opinion has been adopted by some of the more modern physiologists; (a) so late as the year 1778, Sabatier, both from observations made upon human subjects who had received wounds in the thorax, and from experiments performed for the express purpose upon dogs, concludes that the action of the intercostals, both external and internal, tends to contract the chest, and that they therefore belong to the muscles of expiration. (b) The contrary opinion, that both sets of intercostals serve for inspiration, was first started by Mayow; (c) Haller strenuously defended it, and at present this opinion is generally received. He forms a comparative estimate of the increase of capacity which the thorax receives from the action of the diaphragm and of the intercostals. (d) The same author remarks, after Winslow and Senac, that the action of the intercostals is principally useful in those cases where, from any accidental cause, the contraction of the diaphragm is impeded.

From an accurate inspection of the skeleton, and from observations made upon the living body,

(a) Hoadley's *Lectures*, p. 8. (b) *Mem. Acad.* 1778.  
 Richerand, *El. phys.* p. 138. (c) *Tract. quinque*, p.  
 278, & seq. (d) Boer. *Prælect. t. v. pars i. p. 84—9*;  
*El. phys.* viii. 4. 6.



body, anatomists are, for the most part, agreed in supposing, that there is some difference in the manner in which the function of respiration is carried on in the male and female sex; the former appearing to make more use of the diaphragm, and the latter of the muscles about the thorax. (a) It is scarcely necessary to remark, that by this variation of structure, the impediments to respiration, which would otherwise have been produced by a state of pregnancy, are in a considerable degree obviated.

Winflow seems to have committed a mistake in speaking of the opinion of Fabricius, respecting the intercostals; he says, "Ab aquapendente  
 " (Fabricius) avoit dès l'an 1599, très bien de-  
 " montré, que les muscles internes & externes  
 " concourent ensemble à la même action, c'est-a-  
 " dire, à lever les côtes." (b) Fabricius himself employs the following words: "Quapropter dica-  
 " mus nos musculos externos intercostales costa-  
 " rum dilatare, internos vero contrahere, pluri-  
 " bus adducti tum auctoritatibus, tum rationi-  
 " bus." (c) And again, "Concludendum igitur  
 " est externos intercostales thoracem dilatare, in-  
 " ternos vero contrahere." (d) Haller has also

M 2

fallen

(a) Boer. *Prælect.* t. v. p. 145. (b) *Mem. Acad.* 1738.

(c) Fabricii, *Op.* p. 176. (d) *Ditto*, p. 177.



fallen into the same inaccuracy with respect to this author. (a)

### Note VIII.

The celebrated Boyle appears to have been the first, who pointed out the real cause of the influx of the air into the lungs, when the capacity of the thorax is increased. (b) Some good observations were also made upon this subject by his contemporaries, Sylvius, (c) Borelli, (d) and Mayow. (e) Succeeding physiologists for the most part acquiesced in the opinion of Boyle, though some of them, not contented with his simple explanation, have perplexed by their subtle hypotheses, a subject in itself sufficiently intelligible. (f) The process is briefly, yet clearly, detailed by Haller in the following sentence. “ Si  
“ pulmonem, & una pectus, dilataveris, ut aer  
“ pulmonalium vesicularum in amplius spatium  
“ diffusus rareseat, pro portione, qua is aer at-  
“ mosphæra incumbente rarior fit, pro ea co-  
“ lumna aerea in eum pulmonem descendet, eo  
“ celerius,

(a) *El. phys.* viii. 1. 14. (b) *Boyle's Works*, v. i. p. 99. & seq. (c) *Franc. de le Boe Sylvii Opera*, p. 16. (d) *De Motu Anim.* p. 2. prop. 82, 83. (e) *Tract: quinque*, p. 271. & seq. (f) *Baglivi Op.* p. 454. *Hoadley's Lectures*, p. 12. *Bremond, Mem. Acad.* 1739.

“ celerius; quo differentia densitatum major  
“ erit.”

The first operation therefore is the expansion of the thorax, which is effected by muscular contraction; as there is no air between the pleuræ, the lungs act as if suspended in a vacuum; they consequently follow the expansion of the thorax, and the external air rushes into the vesicles, as into the partially exhausted receiver of an air pump, when a communication is formed with the atmosphere. The air enters the lungs because they are expanded; the lungs are not, as was formerly imagined, pushed open by the force of the air rushing into them. (a)

#### Note IX.

Many of the older anatomists supposed, that the lungs were endowed with a certain innate power of motion, by which the alternations of expansion and contraction might be carried on, independently of the change in the capacity of the cavity containing them. They even attempted to establish this position by experiment; but in these instances there must have been some fallacy.

(a) *Boer. præl. t. v. pars i. p. 15. 49, 50.*

fallacy. We know of no method in which animal motion can be produced, except by the contraction of the muscular fibre, and though a muscular structure in the vesicles has been confidently asserted by some writers of eminence, (a) later and more accurate observations have decided against its existence. Helvetius has even endeavoured to prove, that the fibres which are observed between the rings of the trachea, and the larger branches of the bronchia, are not muscular, (b) but their muscularity is admitted by Winslow, Haller, and most other eminent anatomists. (c) The lungs, in common with other bodies composed of cellular texture, possess a degree of elasticity, by means of which, when stretched beyond a certain limit, they will re-act and expel part of their contents. In the process of respiration, however, they are entirely passive, as they always remain in complete contact with the parietes of the thorax.

Notwithstanding these considerations, which have very generally induced the modern physiologists to discard the idea of motion, originating

(a) Willis, *pharm. Rat.* p. 9. Malpighi, *Phil. Trans.* 1671. Bremond, *Mem. Acad.* 1739. (b) *Mem. Acad.* 1718. (c) Winslow's *Anat. Sect.* 9. §. 133. Haller, *Phys.* viii. 2. 12.

in the lungs themselves, it appears, from the following passages in the *Zoonomia*, that Dr. Darwin still adheres to the old opinion, as he directly attributes their action to a cause independent of the contraction of the diaphragm or muscles of the thorax. “ By the stimulus of the blood in  
 “ the right chamber of the heart, the lungs are  
 “ induced to expand themselves, and the pecto-  
 “ ral and intercostal muscles, and the diaphragm,  
 “ act at the same time by their associations with  
 “ them.” (a) And again, “ to these increased  
 “ actions of the air cells, are superadded those of  
 “ the intercostal muscles and diaphragm, by irri-  
 “ tative association.” (b)

M. Dumas also adopts the same opinion, and conjectures, that the lungs are possessed of an innate power of motion; he even expresses his concurrence in the hypothesis of M. Bremond, who inferred from one of his experiments, that the contraction and dilatation of the lungs alternated with that of the thorax, and consequently must depend upon some other cause. (c)

When the lungs are removed from the body,  
 and

(a) *Zoonomia*, v. i. p. 40.

(b) *Ditto*, v. ii. p. 50.

(c) *El. phys.* t. iii. 440, 1.



and the passage into the trachea remains open, they gradually become collapsed, and are contracted into a smaller space than they occupied whilst in the cavity of the thorax. This has been generally ascribed to the re-action of their elastic cartilages and membranes, which, in consequence of the pressure of the internal air, not being balanced by any air between the pleuræ, had been, while in the body, retained in a state of overdistention. Boerhaave and his commentator Haller, attribute part at least of this effect to the contraction of the muscular fibres of the trachea and bronchia; (a) but it may be observed in opposition to such great authorities, that the contractile power of the lungs, remains for a long time after their removal from the body, and cannot therefore with propriety, be ascribed to a cause which must cease with the vitality of the part.

#### Note X.

An elaborate account of these muscles, and of the effects produced upon the thorax by their contraction, will be found in *Boer. prælect. t. v. pars i. p. 126—144*, and *Haller viii. 1. 17—31*.

The

(a) *Boer. prælect. t. v. pars i. p. 6.*



The most eminent anatomists have differed in opinion, respecting the degree of motion which the ribs exercise in respiration; while some authors, with Borelli (*a*) have conceived the action of the intercostals to be essential, others have adduced instances in which the cartilages of the ribs were completely ossified, without any consequent impediment to this function. (*b*) It is also acknowledged, that in the most accurate observations and experiments upon the living body, the motion of the ribs was scarcely visible to the eye. Upon the whole, it seems the most probable, that they are nearly at rest in ordinary respiration, but that in the most violent actions of the thorax, or in cases where the contraction of the diaphragm is impeded by accident or disease, they are employed to compensate for the deficiencies of this organ. But though there has been some difference of opinion respecting the degree of the motion of the ribs, and the occasions on which

(*a*) Borelli, p. ii. prop. 84.      (*b*) Winslow Mem. Acad. 1738; Fabricius de Resp. c. x. sub finem; Haller quotes Cheselden, osteog. c. iii. as recording some cases where no injury was produced to the respiration by anchyloses of the ribs; but this appears to be an oversight; Cheselden says, "I have twice found them" (the cartilages which join the ribs and sternum) "totally ossified in men between 40 and 50 years of age, both of which died with a great difficulty of breathing."

which it is exerted, it is unanimously acknowledged, that the mechanism of their articulations is peculiarly admirable. According to the observation of Bellini, the thorax is increased in all its dimensions, while its figure remains unchanged. (a)

The authors who have treated this subject with the most accuracy are, *Borelli*, p. ii. prop. 81—95. *Senac*, *Mem. Acad.* 1724. *Winslow*, *ibid.* 1720 & 1738. *Boer. Instit.* §. 615 ; *prælect. passim.* *Haller*, *phys. lib. viii. passim.* *Dumas*, *El. phys. t. iii. p. 429—434.*

#### Note XI.

An account of the various estimates which were formed by the older physiologists may be found in *Haller*, *El. phys.* viii. 4. 6.

#### Note XII.

These estimates were formed from persons who had died of disease, the capacity of the lungs after hanging he found to be considerably greater; the average taken from experiments made

(a) *Lemm.* 11.

made upon three bodies, was 260 cubic inches. The author, however, observes, that fear always produces a deep inspiration, and that on this account, in consequence of the peculiar circumstances of their death, their lungs would be in a state of extraordinary distention. (a) Mr. Coleman, on the contrary, from an experiment made for the express purpose upon a dog, concludes, that after hanging, the lungs contain only a very small quantity of air. (b) During the violent struggles which precede death, he supposes that the animal retains the power of expelling air from the trachea, though the pressure of the cord prevents any from being received. My friend Dr. Skey, in order to obviate the apparent contradiction between Dr. Goodwyn's and Mr. Coleman's experiments, suggested that the lungs of a man after hanging must contain more than their usual quantity of air, because from the effects of terror, a full inspiration would be made immediately before death, and the air thus received must be, for the most part, retained by the pressure of the cord, while in an animal unconscious of its approaching fate, it will be a matter of accident whether the lungs were in a state of inspiration or expiration at the moment of death. But I think that

(a) Goodwyn, p. 25, 26.

(b) p. 97, 8.

that this idea, though ingenious, is scarcely sufficient to remove the difficulty. The capacity of the lungs in the three criminals examined by Dr. Goodwyn, coincides very nearly with the estimate of the ordinary capacity of the thorax, deduced from another method of calculation. We must therefore either suppose, that in hanging, the compression of the trachea is produced so suddenly as to retain the thorax in its ordinary state of distention, or if we conceive that an additional quantity of air is received from the effects of fear, we must conclude that it is expelled at the moment when the sense of suffocation first begins to be experienced, but that the passage being almost instantly closed, prevents the farther evacuation of the lungs. Upon the whole, I am inclined to the latter supposition.

With respect to Mr. Coleman's opinion, he informs us that "this experiment was often repeated, and sometimes scarcely any air could be expressed from the lungs." "In all instances, the quantity of air that remained was very inconsiderable." The uniformity of the results seems scarcely compatible with Dr. Skey's supposition. Mr. Coleman states the following propositions among the general conclusions which he deduces from his experiments.

“ 26. That where respiration is suspended,  
“ from ordinary hanging, the animal has the  
“ power of expelling air from the lungs.

“ 27. That although the muscles of expira-  
“ tion perform their office, no power can be ap-  
“ plied to open the trachea to admit air.

“ 28. That as no air can be received, the  
“ animal dies with the same collapse of the lungs  
“ from hanging as from drowning.”

In speaking of the capacity of the thorax in its different states of distention, Mr Davy remarks that, “ Dr. Goodwyn, in his excellent work on  
“ the connexion of life with respiration, has de-  
“ tailed some experiments on the capacity of the  
“ lungs after *natural expiration*. He makes the  
“ medium capacity of the lungs about 109 cubic  
“ inches, which agrees very well with my estima-  
“ tion.” (a) But this statement of Dr. Goodwyn’s opinion is not correct; in the part to which Mr. Davy refers, Dr. Goodwyn says, “ we shall  
“ for the present adopt the medium quantity of  
“ these latter experiments, and say that the lungs  
“ of the human subject contain 109 cubic inches  
“ of air after *complete expiration*.” (b)

Note

(a) *Researches*, p. 411.

(b) *Goodwyn*, p. 27.



## Note XIII.

I have fixed the quantity of air which can be expelled from the lungs, after an ordinary expiration, at 170 cubic inches, because this is nearly the average of the estimates which have been given by different authors, and also because I find, that it coincides with some trials which I have myself made upon the lungs of different individuals. The opinion of Mr. Kite on this subject, seem to be evidently incorrect. "I find," he says, "by repeated experiments, that a person in health, and in a state of perfect rest, usually respires about 17 cubic inches of air; but, at the end of the expiration, there still continues in the lungs 87 cubic inches," &c. (a) From the above quotation it appears, that he either entirely overlooked the quantity which we are now investigating, or confounded it with the air remaining after a complete expiration.

## Note XIV.

Mr. Davy indeed remarks, that "as most gases, though of different specific gravities when brought into contact with each other, assume

(a) *Essays*, p. 47.

“ assume some sort of union, it is more than  
“ probable, that gas inspired into the lungs, from  
“ being placed in contact with the residual gas  
“ on so extensive a surface, must instantly mingle with it.” (a) But in opposition to this argument, we have the direct experiments of M. Jurine, who found the carbonic acid to exist in very different proportions in the different parts of the bronchial vessels. (b) Yet the gases naturally contained in the lungs, possess many circumstances favourable to their equable diffusion, which would not operate in the case of the hydrogen respired by Mr. Davy. The perpetual motion which the lungs experience, the gradual production of the carbonic acid, the length of time during which it remains in contact with the other gases, would all tend to promote its uniform mixture with them. It may likewise be remarked, that carbonic acid, though considerably heavier than the constituents of the atmosphere, differs less from them in specific gravity than the hydrogenous gas. From the experiments of Fontana, we learn also, that hydrogenous gas, even when mixed in a glass jar with oxygen or common air, does not diffuse itself equally through the vessel, but remains in a greater

(a) *Researches*, p. 506—7.  
t. i. p. 494.

(b) *Enc. Meth. Médecine*,

greater proportion in the upper part of it. (a) Dr. Higgins informs us, that when an animal is confined for some time in a jar of air, the carbonic acid is found to exist in greater quantity in the lower than in the upper part of the vessel. (b) In the two experiments on the respiration of hydrogen, related by Mr. Davy, there is a difference in the results of about  $\frac{1}{3}$  part; (c) when the larger quantity of hydrogen was employed, the capacity of the lungs appeared greater, because a larger quantity of the gas being forced into the vesicles, a greater proportion of the residual air was necessarily expelled.

With respect to Mr. Davy's estimate of a single inspiration, although in the experiments made with the mercurial apparatus, he fixes it at 13 inches, in another part of the essay, he informs us, that his lungs in a state of natural inspiration, contain 135 inches, and of natural expiration 118 inches; but the difference between these quantities, which will measure the bulk of a single inspiration, is 17 cubic inches. (d)

#### Note XV.

Borelli remarks, that both from the shape and texture

(a) *Phil. Trans.* 1779. p. 358. (b) *Minutes of a Society*, &c. p. 136. (c) *Researches*, p. 402--6. (d) *Researches*, p. 410.

texture of the bronchia, and the manner in which the diaphragm acts, it is evident that the lungs cannot expel nearly all the air which they contained. (a) Haller conceives it impossible that the lungs can be contracted even to one half of their natural dimensions, unless when removed from the thorax, and deprived of air by the operation of boiling. (b)

# Note XVI.

The tendency which water has to assume the aeriform state, when it is freed from external pressure, is well illustrated by M. Gay-Lussac (c) and by Mr. Dalton, (d) in their respective essays upon the dilatation of the gasses. From the experiments which were performed by many of the older physiologists upon different animal substances placed in the vacuum of the air-pump, we learn, that the blood and the mucous secretions possess the power of extricating gas in a much greater degree than water.

# Note XVII.

This method of explaining the results of Mr.

N

Coleman's

(a) *De Motu Anim.* p. 2. prop. 94. (b) *Phys.* viii. 4. 11.

(c) *Ann. de Chim.* t. xliiii. (d) *Manchester Mem.* v. v.

Coleman's experiments was, I believe, first pointed out by Mr. Allen in his lectures.

### Note XVIII.

In estimating the bulk of a single inspiration at 40 cubic inches, I have been principally influenced by the experiments of Dr. Menzies, as they appear to be the most accurate in their execution, the least exceptionable with respect to the nature of the processes employed, and the most numerous and varied of any that have been performed upon this subject. In favor of this opinion we have also the authority of Jurine, Sauvages, (a) Hales, (b) Haller, (c) Blumenbach, (d) Chaptal, (e) and Bell; (f) Richerand, (g) also estimates the bulk of a single inspiration at between 30 and 40 cubic inches, and Fontana (h) at 35.

### Note XIX.

There is scarcely any opinion in physiology, however absurd it may at first view appear, which has not found some supporters; and accordingly it

- (a) Haller, viii. 4. 6. (b) *Statistical Essays*, v. i. p. 238. (c) *El. Phys.* viii. 4. 6. (d) *Instit. Phys.* p. 113. (e) *Chemistry*, v. i. p. 133. (f) *Anatomy*. v. ii. p. 193. (g) *Phys.* p. 147. (h) *Phil. Trans.* 1779. p. 349.



it appears that arguments have been urged to prove that the foetus breathes whilst in the uterus. (a)

### Note XX.

This is the hypothesis which is advanced by Haller in the *El. Phys.* viii. 5, 2; in his notes upon Boerhaave, which were published at an earlier period, he supposes that the first respiration is produced by the cry of the animal, in consequence of the unpleasant feelings which it experiences immediately after birth, from cold, pressure, &c. (b) This, however, cannot be considered as any explanation of the phenomenon, as it is evident, that there must have been a previous inspiration before the cry could have been produced.

### Note XXI.

I consider an instinctive action to be one which is attended with sensation, and directed to some useful purpose, but which has been derived neither from experience, observation, nor tradition.

### Note XXII.

Though it is undesirable to occupy the attention

N 2

with

(a) Haller, xxix. 4. 54; *Whytt on Vital Motions*, sect. 9. *Boyle's Works*, v. i. p. 110. (b) *Prælect. t. v. p. ii. p. 456.*

with exploded theories, yet as a part of the history of science, it is in some degree necessary, and in many cases highly interesting, to take a cursory view of the doctrines that have been supported by men of celebrity, and which for a time have enjoyed a share of popularity. For this reason I shall subjoin a short sketch of several different hypotheses, that have been formed to account for the first inspiration, and for the subsequent alternation of inspiration and expiration, besides those, which, in consideration of their own superior merit, or the more distinguished celebrity of their respective authors, I have thought proper to admit into the body of the work.

1. With respect to the cause of the commencement of respiration;

Borelli imagined that the blood in its passage through the lungs, received from the air certain particles, which by communicating to it a vibratory or oscillatory motion, adapted it for the purposes of life, and rendered it proper for supporting the circulation. The blood of the foetus, he supposed, acquires these particles from the blood of the mother, but that as soon as the communication is destroyed between them, it becomes necessary

cessary for the same action to be performed by the lungs of the young animal itself. (a)

Pitcairne conceived it to be a general law of the animal œconomy, that a muscle which is without an antagonist, must perpetually undergo an alternation of relaxation and contraction, and as the force of the muscles of inspiration so far exceeds that of the muscles of expiration, he considers the diaphragm and intercostals as without antagonists, and applying his general hypothesis to the peculiar case of the action of the thorax, he supposes that the alternate action of the diaphragm must commence immediately after birth. (b)

Petit conjectures, that the animal spirits are, from the first existence of the fœtus, flowing downwards into the muscles of respiration, and tending to produce their contraction; but that in consequence of the pressure which the animal experiences while in the uterus, these muscles are not at liberty to act. As soon however, as the position of the new-born animal permits this effect to be produced, the thorax becomes enlarged from the contraction of the diaphragm and intercostals,

(a) *De Motu Anim. pars ii. prop. 118.*

(b) *Dissert.*

costals, and the air consequently rushes into the vesicles. (a)

Boerhaave imagines it a sufficient solution of this difficulty to state, that the foetus in the time of birth, in consequence of its struggles, puts all its muscles into action, and among others the diaphragm. From this cause the thorax becomes expanded, and the air rushes into it; the blood then flows through the vessels of the lungs, and respiration ever after proceeds as in the adult animal. (b)

The method employed by Dr. Hartley to account for the commencement of respiration, bears a considerable resemblance to the hypothesis of Boerhaave. He supposes that respiration and crying are excited in the new-born child from the cold, handling of the midwife, and other vivid sensations, impressed immediately upon its coming into the world. These sensations tend to throw the whole system of muscles into action, and in those cases, where there are antagonists, the stronger will overcome the weaker, and contraction will be consequently produced in them. (c)

According

(a) *Mem. Acad.* 1733. (b) *Instit.* §. 691. (c) *Observations on Man*, v. i. p. 95.

According to M. Buffon, when the animal leaves the uterus, the air acts upon all the organs of sensation, but particularly upon the olfactory nerves. They are by this means stimulated, and the animal makes an attempt to sneeze; the cavity of the chest is thus enlarged, and the air has liberty to flow into the lungs, and the vesicles become dilated. But the air which is received into them has its temperature increased, and being expanded, causes the re-action of the fibres of the vesicles, which forces it again out of the chest. (a)

Prof. Blumenbach supposes, that upon the stoppage of the circulation between the mother and the foetus, a sense of suffocation is experienced by the latter. This circumstance, together with the external cold, and the other new stimuli to which the body is exposed, causes a variety of motions to take place, and among others the dilatation of the chest, and thus produces the first inspiration. (b)

2. With respect to the immediate cause of the successive alternations of inspiration and expiration;

Some

(a) *Nat. Hist. t. ii. p. 446.*

(b) *Instit. Phys. p. 117*



Some of the attempts that have been made to resolve this problem, as those of Willis and Pitcairne, have proceeded upon the assumption of principles, altogether inconsistent with the actual laws of the animal œconomy. The former of these authors conceived, that during muscular contraction, the animal spirits are conveyed from the tendon to the fibres of the muscle itself, and in the subsequent relaxation return into the tendon. In the animal functions this operation is supposed to be under the power of the will, but in the muscles subservient to the natural functions, the changes are effected independently of the will, in regular alternation. He attributes the difference in the operation of these parts, to the origin whence they derive their nerves, which in the former case proceed from the cerebrum, and in the latter from the cerebellum. (*a*)

Pitcairne imagined that all muscles which are without antagonists, are necessarily in a perpetual state of alternate contraction and relaxation, an effect which he ascribes to the interrupted influx of the animal spirits into these muscles, in consequence of the alternate compression which the brain experiences from the dilatation of the arteries. (*a*)

The

(*a*) *Pharm. Rat.* p. 18.

(*b*) *Dissert.* p. 62.

The learned mathematicians Borelli and Bellini, simply state the necessity of this regular action to the existence of the animal, and appear to rest satisfied with pointing out its final cause. (a)

Other physiologists have endeavoured to account for the phenomenon, by some circumstance in the structure, or mechanical action of the thorax, or of the parts contained in it. Dr. Martine explains the action of the organs of respiration, by supposing, that the lungs, when in a state of distention, compress the phrenic nerve, and thus produce a relaxation of the diaphragm and intercostals, to which these nerves communicate sensibility. (b)

A peculiar theory was adopted by Boerhaave to explain this phenomenon; he imagined that in the state of full inspiration, the lungs were so much distended as not to permit the blood to be transmitted through them with its accustomed facility. Less blood will therefore be received by the left ventricle, and consequently less sent to the cerebellum, and to the nerves which are distributed to the organs of respiration; hence arises a deficiency

(a) Borelli, p. ii. prop. 117. Bellini, Lem. 18. (b) Edin. Med. Essays, v. i.

a deficiency of the action of those parts, and a consequent relaxation of the muscles which serve to increase the size of the thorax. The distended state of the thorax being removed, the blood has liberty to flow freely into the left ventricle, and is consequently sent in its proper proportion to the cerebellum, it therefore excites the muscles of inspiration to contract, and brings back the lungs to their former condition. (a)

Dr. Hartley applied his doctrine of vibrations to the solution of this problem. He conceives that his peculiar theory explains in an easy manner, the propensity to alternate contraction and relaxation, which is observed in many of the muscles, and upon this general principle, he endeavours to explain the action of the diaphragm; he however attributes the effect in part to association or habit, and in part also to impressions made upon the pleura and peritonæum, exciting vibrations in them which are communicated to the diaphragm and muscles of the abdomen. (b)

I have stated the opinion of Professor Blumenbach, respecting the cause of the first inspiration;

(a) *Instit.* §. 619, 620. (b) *Observ. on Man*, v. i. p. 96.

ration ; in consequence of this action, the lungs become dilated, and the blood flows through them. The air and the blood then act upon each other, and the former of these substances undergoes that peculiar change, which renders it unfit for the farther support of life. It is then discharged from the lungs, in consequence of the efforts of the vis medicatrix naturæ, “ naturæ medicatricis conaminibus,” and makes room for the influx of a new portion of air. (a)

Dr. Darwin has not formally proposed any hypothesis, to account for the alternations of respiration, but it may be inferred from some expressions in the Zoonomia, that he considered the actions of inspiration and expiration to succeed each other in consequence of association. To this explanation of the phenomenon, it may be objected, that according to the definition of the author himself, a complicated series of actions can only become associated together “ *by habit ; i. e. by frequent repetition.*” (a) But the young animal, from the first instant of its birth, possesses as complete a power over the muscles of respiration, as it ever acquires during the course of its existence. Those cases, in which for a few

(a) *Instit. phys.* p. 117.

(b) *Zoonomia*, v. i. p. 13.

few moments, a difficulty has occurred in the commencement of respiration, are to be referred to a mechanical obstruction in the trachea, preventing the access of the air, or to some other accidental circumstance, for they are by no means of frequent occurrence, and they are in general immediately relieved by a change of posture, or by the removal of mucus from the fauces. The actions which are acknowledged to be composed of a number of associated motions, such as dancing, or playing upon a musical instrument, though many of them less complicated than the act of respiration, can only be acquired by long practice, and by repeated efforts of attention.

### Note XXIII.

The celebrated Harvey, seems to have been the first who stated, that the action of the heart and lungs are totally independent of each other. He however imagines, that the passage of the blood along the pulmonary vessels, is assisted by the motion of the thorax. (a)

The connexion which was supposed by some of the older writers, to exist between the motions  
of

(a) *De Motu Cord.* p. 11. 71. 77.



of the heart and the diaphragm, depended upon the structure or mechanical action of these organs. The learned author of the *Zoonomia*, who has adopted the idea of this connexion, has deduced it from that general law of the animal œconomy, by which motions that are frequently repeated in succession, acquire the power of recurring in the same order, independently of the original exciting cause. Dr. Darwin observes, that “innumerable trains or tribes of other motions are associated with these muscular motions which are excited by irritation; as by the stimulus of the blood in the right chamber of the heart, the lungs are induced to expand themselves.” (a) But it may be remarked, in opposition to this supposed association, between the motions of the heart and the lungs, that in the foetus the heart commences its contractions immediately upon its formation, while the lungs remain perfectly at rest. When the animal leaves the uterus, the motion of the lungs commences, but the periods of the contraction of the diaphragm bear no determinate ratio to those of the systole of the heart. The frequency of the pulse is often increased or diminished far beyond its accustomed standard, while little alteration is produced in the motion of

(a) *Zoonomia*, vol. i. p. 40.

of the diaphragm. The action of both these organs, however, depends upon the quantity and the quality of the blood transmitted to them, and any affection of this fluid, will of course influence both the circulation and the respiration. These functions also both depend ultimately upon the contraction of the muscular fibre, and are therefore equally liable to experience an alteration, from any circumstance which affects the state of the irritability or the sensibility of the system. But it is obvious that these changes, though contemporary and proportional, do not depend upon association; they are produced by an assignable cause, operating directly upon each separate organ, distinct from the effects of repetition or habit. Were it not foreign to my present subject, it would not, I think, be difficult to shew, that a similar objection may be urged against many parts of the physiology and pathology of the Zoonomia, and particularly against the sympathetic theory of fever.

Mr. Hunter, supposes the existence of this sympathy, or association, between the motions of the heart and the lungs, (a) and the same opinion appears to be adopted by Dr. Currie. (b)

Note

(a) *Hunter on the Blood*, p. 54. (b) *Med. Reports*  
p. 75.

Note XXIV.

Helvetius, Senac, Winflow, &c. Boerhaave, however, conceived that the lungs were rendered less capable of transmitting the blood during the state of full inspiration, and from the supposed effects of this retardation, he deduced his hypothesis to account for the alternations of respiration.

Note XXV.

The celebrated experiment exhibited by Hooke, before the Royal Society, in which, after the motion of the heart had ceased, it was reproduced by inflating the lungs, has been adduced to prove, that by inspiration the lungs are rendered pervious to the blood, (a) but it must be considered that in this case, the thorax was laid entirely open, and consequently, when the lungs were not forcibly distended, they would collapse so much as almost entirely to obliterate the cavities of the bronchia and vesicles.

Note XXVI.

Morgagni, relates a case, in which, upon examining

(a) Haller, viii. 4. 12.

aming after death, a considerable quantity of serous fluid was found in the cavity of the thorax, yet the pulse had not been previously affected, nor had any impediment to the circulation been observed during the life of the patient. (a)

#### Note XXVII.

The experiments and observations of Mr. Coleman, upon the impermeable state of the lungs in drowned animals, will more properly fall under our consideration in a subsequent part of the essay.

#### Note XXVIII.

It has been observed, that when from any accidental cause, a portion of the cranium is removed, an alternate elevation and depression of the brain is visible to the eye, corresponding with expiration and inspiration. This effect has been attributed to the resistance which the blood experiences to its passage through the lungs in a state of expiration; a stagnation is thus brought about in the right side of the heart, and consequently a less free discharge from the veins, on which

(a) *Seats and causes of Diseases*, v. i. p. 408, 9.

which account, in a highly vascular part, an increase of bulk will be produced ; this is removed by the depletion of the veins, when the lungs from the enlargement of the thorax become again more completely pervious to the transmissiion of the blood. There seems no reason to doubt that this phenomenon has been occasionally observed, and it is probable that it depends upon the cause assigned above. But it must be remarked, that in those instances, in which so great an injury is received by the cranium, the respiration is necessarily rendered laborious, the air is received into the lungs after long intervals, and consequently in an increased quantity. (a)

### Note XXIX.

It has been asked, why are the lacteals furnished with valves, while the veins which pass along the same viscera, and are conveying their contents in the same direction, are not provided with them. One cause of this difference in structure is obvious ; the chyle, not being possessed of any vis a tergo when it enters the lacteals, is propelled along them solely by the contraction of their muscular fibres, and the effect of this

O *muscular*

(a) *Haller*, vi. 4. 9; viii. 4. 11. 27.



muscular contraction is evidently increased by the presence of the valves. When, on the contrary, the blood enters the abdominal veins, it retains a degree of velocity which is sufficient to carry it along them, and though its progress might have been accelerated by the addition of valves, we must conclude that its present velocity is the best adapted to the wants of the system.

### Note XXX.

I consider the absolute neglect into which the works of Mayow had fallen in this country, until they were again brought to light about 20 years ago, as one of the most curious circumstances in the history of modern science. He lived at a period when the cultivation of natural philosophy was beginning to be popular, and after it had been sanctioned by several illustrious examples. The atmosphere had also been made a particular object of attention, and the discovery of the air-pump had enabled Boyle and others to make very important advances in the knowledge of its properties. Mayow's style is sufficiently clear and perspicuous; although there are many hypotheses advanced in his essays, some of which are founded upon the false philosophy then prevalent, yet they contain less theory and more experiments

experiments, than the writings of almost any of his contemporaries. The neglect of Mayow's works will appear more singular, when we consider, that they issued from the press of Oxford, of which University he was himself a member, and that at the time of their publication, they appear to have been pretty generally read, and to have made as much impression as could, at that period, be reasonably expected. They passed through three or more editions in the space of a few years, the whole or a part was republished on the continent, and they are frequently quoted by the physiologists and anatomists, who lived about the conclusion of the 17th century. By some strange and unaccountable fatality, they afterwards sunk into almost complete oblivion, and more especially in his native country, they came at length to be absolutely forgotten. He is only twice referred to by Dr. Hales,<sup>(a)</sup> and then in such a manner, as to render it doubtful whether he had seen his works, and in the discourse delivered by Sir John Pringle before the Royal Society, upon the assignment of Sir Godfrey Copley's medal to Dr. Priestley, which commences with a sketch of the discoveries that had been made in the science of aerology, previous

(a) *Statical Essays*, v. i. p. 234 & 236.

to the period when this philosopher entered upon his experiments, the name of Mayow is not mentioned. (a)

On the continent, although little estimated, and seldom referred to, his works were occasionally quoted, and Haller in particular, certainly appears to have been acquainted with them. They were first again brought into notice in this country by Dr. Reinhold Forster, who prefixed a short account of Mayow's doctrines, to his translation of Scheele's essay on air and fire.

The vicissitudes of Mayow's reputation, afford a striking instance of the tendency which exists in the human mind, to fly from one extreme to the opposite excess. When his works were thus brought before the public, his cause was eagerly embraced by different writers, his merits were exalted to the highest rank, and it was asserted, that he had anticipated the most valuable discoveries of Scheele, Priestley and Lavoisier. (b) But a calm investigation of the subject, will, I conceive, lead us to a very different conclusion. We shall,

(a) *Discourses*, p. 1—20. (b) See particularly Dr. Beddoes's pamphlet, entitled, "Chemical experiments and opinions, extracted from a work published in the last century."

shall, indeed, find in this author many marks of real and original genius, and it is evident, that on some points, he extended his views beyond those of any of his contemporaries. It must, however, be confessed, that the real addition which he made to the general stock of knowledge was not very great. He endeavoured to prove, that there was something in the atmosphere, which was an immediate agent in the processes of combustion and respiration; but this had been long ago asserted by others, and when he comes afterwards to explain more in detail, his ideas respecting the intimate nature of this peculiar matter, he evidently shews that he had not any conception of the substance to which we now ascribe these phenomena. He is even doubtful, whether it forms an essential part of the air, and upon the whole inclines to the negative side of the question. In order to account for the alteration produced in the air by its removal, he adopts an hypothesis to the last degree fanciful, according to which it consists in some mechanical change in the configuration of its particles, which principally operates in diminishing its elasticity. He imagines that this matter enters into the composition of the nitrous, and other acids, and that it gives them the peculiar properties which belong to this class of bodies, but he  
also



also supposes, that the same substance is a component part of the alcalis, and is the immediate cause of their causticity, and thus decisively proves that he had no accurate conception of the modern doctrines respecting the constitution of acids.

As to the subject of respiration in particular, when speaking of the action which the air exercises upon the blood, he has recourse to the exploded doctrine of fermentation; he supposes that the particles of the air and of the blood, when brought into contact, are rubbed against each other, in consequence of which, the former are deprived of that spirit to which they owe their elasticity. I think that an attentive perusal of Mayow's works will convince the dispassionate reader, that there is no substance which has been discovered by the modern chemists, that can perform the various effects which are attributed by this author to his nitro-aereal spirit.

Perhaps the most important of Mayow's observations, are those respecting the mechanism of respiration, and particularly the very decided manner in which he states that the external and internal intercostals are equally subservient to the enlargement of the thorax. The ingenious experiment, however, where the action of the lungs



is illustrated by comparing them to a bladder inclosed in a pair of bellows, though brought forwards by Mayow as original, (a) is mentioned by Boyle in the most explicit manner, as what was already familiar to every one, and the conclusions which he deduces from it, are precisely similar to those formed by Mayow. (b) There is one department, where the genius of Mayow was peculiarly successful, by which, it is probable, he might have materially promoted the advancement of science, had a greater degree of attention been bestowed upon his writings; I refer to the contrivance of his apparatus and the execution of his experiments. In these respects he may be considered as far excelling any of his contemporaries, and as at least equal to any chemist before the time of Scheele and Priestley.

In forming an estimate of the merits of Mayow, it is essential to observe, that a theory of combustion, almost precisely similar to that adopted by this philosopher, had been a few years before published by Dr. Hooke, of which it is scarcely possible that Mayow could have been ignorant; it is however, not noticed by him. It appears, indeed, that Hooke has the advantage,

(a) *Tractatus quinque*, p. 274, 5.

(b) *Works*, vol.

i. p. 101.

tage, not only with respect to originality, but likewise in the accuracy and simplicity of his hypothesis. According to Professor Robison, Mayow's exhibition of Hooke's doctrine is, "obscure, complicated, and wavering, mixed with much mechanical nonsense, of wedges, and darts, and motions, according to the fashion of the times. Hooke's conception of the subject, is clear, simple and steady." (a) Dr. Thomson passes a similar judgment upon Mayow; he bears ample testimony to the originality and accuracy of his experiments, but he adds, that "his reasoning is for the most part absurd, and the additions which he made to the theory of Hooke are exceedingly extravagant." (b) Dr. Yeats has treated largely upon the writings of Mayow, but has, in my opinion, very far over-rated their merit. He expressly attributes to him the discovery of oxygene, and supposes, that he was accurately acquainted with all its properties. (c) M. Fourcroy has published a candid and judicious account of Mayow in the *Enc. Method.* (d) which was afterwards inserted in the *Annales de Chimie.* (e) In examining the writings

(a) *Black's Lectures by Robison*, v. i. p. 535, 6.

(b) *Thomson's Chemistry*, v. i. p. 347.

(c) *Observations on the claims of the Moderns, &c.* p. 14, & alibi.

(d) *Chimie*, t. iii. p. 390. (e) t. xxix.

ings of the older chemists and physiologists, it is necessary to remember, that expressions which appear to coincide remarkably with the modern discoveries, would not suggest the idea of these discoveries to one who was previously unacquainted with them. It has accordingly happened in several instances, that the modern doctrines have been supposed to be detected in books, where their existence was not suspected, until these doctrines had been deduced from the experiments and reasonings of the chemists of the present period.

### Note XXXI.

Dr. Black made this discovery by breathing through lime water, or the solution of a caustic alkali, when he found that the lime was precipitated, and the alkali rendered mild; effects which he attributed to fixed air emitted from the lungs; he consequently conceived, that the change produced by respiration upon wholesome air, consists principally in the conversion of part of it into fixed air. It appears that he made these discoveries in the year 1757, and it may be inferred, that they were about the same time publicly announced in his lectures. (a)

Note

(a) Black's Lectures, by Robison, v. ii. p. 87.

## Note XXXII.

Besides the oxygenous and azotic gasses, a small quantity of carbonic acid gas, seldom exceeding  $\cdot 01$ , has been found to exist in the atmosphere, and is so constantly present in all the different situations in which the experiment has been tried, that it probably ought to be considered as one of its constituent parts. (a) The aqueous vapour, on the other hand, is perpetually varying in its proportion, and seems evidently to be an extraneous body, depending for its formation upon accidental causes. The proportion of oxygen which was indicated in the experiments of Scheele and Priestley, and in the more accurate examination of the atmosphere, which was afterwards made by Lavoisier, (b) is greater than that stated in the text. But the process of Eudiometry has undergone of late considerable improvements, and the very accurate experiments of Berthollet, (c) De Marti, (d) and Davy, (e) agree in indicating the proportion of oxygen

(a) *Saussure, voyage dans les Alps*, t. vii. c. 6. p. 2010. *Humbolt, Journal de Phys.* t. xxxvii. p. 202. *Encyc. Meth. Chimie, Air*, t. i. p. 745. (b) *Elem. of Chem.* chap. 3. p. 86. (c) *Ann. de Chim.* t. xxxiv. p. 73 & seq. (d) *Journal de Phys.* t. lii. p. 173. & seq. (e) *Nicholson's Journ.* v. v. p. 175.

gene to azote to be nearly as 22 to 78. These quantities must be considered as referring to the volume of the two gasses; their proportional weights are somewhat different, and will be as 26 to 74 nearly. (a) M. Fourcroy, however, in his late elaborate work, still supposes, that the atmosphere contains  $\cdot 27$  of oxygene. (b)

## Note XXXIII.

I cannot avoid remarking, that in this memoir M. Lavoisier speaks of his observation respecting the carbonic acid gas, as of a fact discovered by himself, nor does he mention the name of Dr. Black, who it appears had publicly taught for twenty years that this aeriform fluid is disengaged by respiration. It is much to be lamented that a man, who had so many real claims to the admiration of posterity, should in any instance have attempted to encrease his reputation at the expense of his contemporaries.

## XXXIV.

This proposition is subject to exceptions; M. Vauquelin found that some of the species of worms possessed

(a) Thomson's *Chem.* v. iii. p. 269.  
*conn. chim. t. i p. 156.*

(b) *Système des*



possessed the power of separating the oxygene from the azote in the most perfect manner. He performed his experiments upon the limax flavus and the helix pomatia.(a)

### XXXV.

The two papers agree very nearly in the weight of oxygene supposed to be consumed in twenty-four hours; the first makes it 19080 French grs. the latter 19090. But though the weights so nearly correspond, the assigned volumes are considerably different; the first memoir states it to be 24, and the latter something more than 22 Paris cubic feet. If we suppose 100 cubic inches of oxygene to weigh 50.5 Paris grs.(b) 24 cubic feet, or 41472 cubic inches will weigh 20943.36 grs.; 22 cubic feet, or 38016 inches will weigh 19198.08 Paris grs. or 15750.33 English Troy grs.

The Paris weights and measures of the original are reduced to the English standard by means of the formulæ given by Professor Robison in Kerr's translation of Lavoisier's Elements, *App. No. 5.*

### XXXVI.

It must be remarked, that at the time when  
Mr.

(a) *Ann. de Chimie*, t. xii. p. 278. & seq. (b) *Kirwan on Phlogiston*, p. 36.

Mr. Davy performed these experiments, he conceived the atmosphere to contain  $\cdot 27$  parts of oxygen; (a) in the more accurate experiments which he afterwards performed, he found the proportion of oxygen to be little more than  $\cdot 21$ . (b) This circumstance will render it necessary to make some alterations in the estimate of the effects of respiration, as we may conclude that the same Eudiometrical process was employed in the analysis of air which had been respired, as of the air of the atmosphere.

Mr. Davy's estimate.

100 cubic inches of atmospheric air before the experiment,

	Oxygene.	Azote.	Carbonic Acid.
Consisted of	27	73	
After the experiment of	22.5	71.7	4.5
Consequently there was			
an absorption of	4.5	1.3	
And a production of			4.5

Corrected estimate.

Before the experiment	21	78	1
After the experiment	17.5	76.6	4.5
Consequently there was			
an absorption of	3.5	1.4	
And a production of			3.5
			Note

(a) *Researches*, p. 327. (b) *Nicholson's Journal*, v. v. p. 175.

## Note XXXVII.

Boyle first attempted to ascertain the amount of the diminution, which the air experiences in respiration; he estimated it at  $\frac{1}{30}$  part of its bulk. Mayow concluded from his experiments, that the air was diminished  $\frac{1}{14}$  part. (a) Hales made many experiments to ascertain this point, but their results differ so considerably, as to render it impossible to fix any general standard; it varied from  $\frac{1}{30}$  to  $\frac{1}{13}$  part of the whole air. (b) All these quantities, however different from each other, agree in being greater than the estimates of the modern chemists. This circumstance is to be attributed to the absorption of part of the carbonic acid formed in the lungs, by the water with which, in all the different experiments, the air of expiration was placed in contact.

## Note XXXVIII.

The celebrated Sanctorius, who devoted so large a portion of his life to the investigation of the quantity of matter perspired from the body, under different circumstances, estimated the pulmonary exhalation, at about half a pound in 24 hours.

(a) *Tract. quinque*, p. 405. (b) *Stat. Essays*, v. i. p. 238; v. ii. p. 320.

hours. (a) Dr. Hales, by pursuing the plan which is stated in the text, supposes the quantity to be above 20 oz. Dr. Home remarks, that the perspiration from the lungs, is "computed" by Sanctorius to amount to 6 oz. in 24 hours, "by Hales to 23 oz.; the latter computation seems too high, part of that moisture being probably owing to what is contained in a dry air." (b) It appears, that in this instance, Dr. Home has been inaccurate in stating the calculation of Hales, and what is more remarkable, Haller, in giving the opinion of Home, says, "ad uncias 23 æstimat Cl. Home," (c) and refers to the passage quoted above.

## Note XXIX.

Dr. Priestley had previously observed this circumstance, and had estimated that  $\frac{1}{3}$  part only of the oxygene consumed in respiration was employed in the formation of the carbonic acid emitted from the lungs; the rest of the oxygene he conceived was absorbed by the blood. (d)

## Note XL.

It must be remembered, that a method of estimating

(a) *Med. Statica*, p. 36. (b) *Medical facts*, p. 238.

(c) *El. Phys.* viii. 5. 40. (d) *Experim. on Air*, v. iii. p. 378, 9.

estimating the quantity of water produced in the lungs, very similar to that made use of by Lavoisier, and founded nearly on the same train of reasoning, had been previously employed by Dr. Crawford; he supposes that about  $\frac{1}{2}$  part of the oxygene consumed, enters into the formation of aqueous vapour. (a)

### Note XLI.

This is not the only instance in which, after the conclusions of Dr. Priestley have been controverted by his contemporaries, a more accurate investigation of the question has ultimately decided in his favour. The complicated apparatus, and the imposing air of minuteness, which characterize the operations of the French chemists, irresistibly engage the assent of the reader, and scarcely permit him to examine the stability of the foundation upon which the structure is erected. The simplicity of the processes employed by Dr. Priestley, the apparent ease with which his experiments were performed, and the unaffected, conversational style in which they are related, have, on the contrary, been mistaken for the effects of haste and inaccuracy. Something must also be ascribed to the theoretical language which pervades

(a) *Observ. on Animal heat*, p. 154, 347, 8.



vades and obscures the chemical writings of this philosopher, in consequence of his unfortunate attachment to the doctrine of phlogiston.

### Note XLII.

Mr. Davy found that 71·7 cubic inches out of 99 which he expired were azote; (a) if therefore we estimate the average contents of the lungs at 280 cubic inches, 213 of these will consist of azote.

### Note XLIII.

In the first of the general conclusions, I have stated, that the atmospheric air received into the lungs, loses about ·04 of the oxygene which was contained in it. Supposing the whole quantity of air respired in 24 hours to be 666·5 cubic feet, the quantity of oxygene which is removed will amount to 26·66 cubic feet. This estimate agrees exactly with the quantity of oxygene which I have supposed to be consumed in 24 hours, deduced from the experiments of M. Lavoisier and Mr. Davy. The quantity ·04 is however calculated from a different set of experiments,

(a) *Researches*, p. 432.

riments, and the number  $666 \cdot 5$  involves the estimate of a single inspiration, and of the quantity of air respired in 24 hours, both which were obtained from different sources.

In the 2d general conclusion it is stated, that 22 cubic feet of carbonic acid gas are generated in 24 hours, so that there is, upon the whole, a loss of 4 cubic feet in 24 hours, so far as respects the consumption of oxygene, and the production of carbonic acid gas. In the 5th general conclusion it is stated, that a quantity of azote is consumed in respiration, amounting to about  $\frac{1}{160}$  part of the whole air employed. Supposing this, as in the former case, to be  $666 \cdot 5$  cubic feet, the azote absorbed, will be about 4 cubic feet. These, added to the 4 cubic feet mentioned in the last sentence, will make the total loss by respiration in 24 hours 8 cubic feet. In the 3d general conclusion, it is said, that the volume of the air respired is diminished by  $\frac{1}{80}$  part; now  $666 \cdot 5$  divided by 80 exactly produces the number  $8 \cdot 33$ . The coincidence in this case is more remarkable than in the former, as it involves a greater number of quantities, all obtained by separate calculations, and deduced from independent sources. It must, I conceive, be not only regarded as a strong argument

ment in favour of their accuracy, but, in some measure, at least, of the hypotheses from which they were deduced.

Note XLIV.

M. Lavoisier states, that 100 parts of carbonic acid consist of 72 parts oxygen, and 28 parts carbon, (a) but it has been discovered since the period of his experiments, that the charcoal which entered into the composition of the acid, is itself previously in the state of an oxide, containing  $\cdot 36$  of oxygen; the composition of carbonic acid will therefore be pure carbon 18, and oxygen 82 parts; or the oxide of carbon which composes charcoal 28, and oxygen 72 parts. (b) Upon the supposition that the carbon in the blood is in the state of the black oxide, there will in the space of 24 hours, be a surplus of oxygen of  $5 \cdot 59$  oz.; but if the carbon be in a state of purity, the superabundant oxygen will be only  $1 \cdot 88$  oz.

Note XLV.

This idea of the nature of the change pro-

P 2                      duced

(a) *Mem. Acad.* 1789, p. 567. (b) *Thomson's Chemistry*, v. i, p. 50.

duced upon the blood by respiration, was founded upon a supposition, that its velocity was greater during its passage through the capillaries of the lungs, than in any other part of the circulation; an opinion which was at one time adopted by several physiologists of eminence. Dr. Hales, who performed experiments expressly upon this subject, informs us that he observed the blood to move five times as quickly through the lungs as through the muscular capillaries. (a) Senac, on the other hand, thinks that its motion through the lungs cannot be quicker, because each of the auricles must receive the same quantity of blood in the same time; (b) and others have supposed that the velocity must be less in the lungs, the pulmonary circulation being altogether shorter than that of the aortic system, the velocity must be as the spaces gone through in the same time. Haller does not imagine that the average velocity in the lungs differs from that in the rest of the body; he however remarks that it is sent through them with less force; because there are fewer obstacles to its progress, and the right ventricle is consequently less strong than the left. (c)

## Note

(a) *Statical Essays*, v. ii. p. 66. (b) *Sur le Coeur*, t. ii. p. 161. (c) *Haller*, viii. 5. 21.

## Note XLVI.

The following remarks of Boerhaave, who was distinguished for his extensive acquaintance with all the branches of science, may serve as a proof of the entire ignorance respecting the change effected in the blood by respiration which prevailed in his time. He imagines that as long as the foramen ovale and ductus arteriosus are kept open, the animal can continue to live without air, because the blood will then be able to pass from the right to the left side of the heart. He even supposes, that by frequently plunging the young animal in water, the closing of these holes might be prevented, and the creature be thus rendered amphibious. Why we cannot dilate the chest under water, and continue to live in this situation, he says, “ hoc nobis *Κρυπτον* est,” but he adds with his accustomed candour, “ quod forsan vobis, vestroque ævo, aliquid patefcet.” (a) M. Buffon actually performed some experiments upon this subject. (b) These opinions, which at present assume almost a ludicrous appearance, were pardonable at the period when they were advanced. But we can scarcely extend our apology to the physiologist, who after all the pneumatic discoveries which

(a) *Prælect. t. v. pars 2. p. 467—474.* (b) *Nat. Hist. v. ii p. 447.*



which have immortalized the names of Black, Priestley, Lavoifier, and Crawford, can still fancy that, "by frequent immersion in water, the association between the movements of the heart and lungs might perhaps be dissolved; and an animal be inured to live commodiously, for any time, under water." (a)

#### Note XLVII.

It is curious to observe the manner in which Haller speaks of this experiment. "Hoc vulgare experimentum non a Lowero solum, verum etiam ab Helvetio serio propositum est."

#### Note XLVIII.

Dr. Crawford, in conformity with the theory at that time prevalent, imagined that carbonic acid was produced by the union of oxygene and hydrogen. He was confirmed in this error by the effects which hydrogen had been found to produce upon the crassamentum in the experiments of Dr. Priestley, where bright scarlet blood, by exposure to this gas, was rendered dark coloured,

(a) Beddoes on fact. airs, part i. p. 41.

coloured, and acquired the venous character. Dr. Hamilton also found that the same change was produced in the colour of arterial blood by introducing hydrogenous gas into the carotid of a living animal. (a)

### Note XLIX.

M. Haffenfratz notices the objections which are mentioned in the text, yet he lays the most stress upon the manner in which the old theory accounts for the production of animal heat. He asserts, that according to it, the caloric is altogether evolved in the lungs, and that consequently their temperature ought to be much superior to that of the other parts of the system. He says, that according to Dr. Crawford's hypothesis, "les poumons sont le foyer où se degage toute la chaleur que le sang abandonne dans l'economie animale." And again, "M. de la Grange reflechissant que si toute la chaleur qui se distribue dans l'economie animale se degageoit dans les poumons, &c." (b) When we consider that this memoir was written many years after the publication of Dr. Crawford's treatise

on

(a) Crawford, p. 147, & seq. (b) *Ann. de Chim. t. ix. p. 266.*

on animal heat, it is somewhat surprising, that such expressions should be employed by so experienced a philosopher as M. Haffenfratz. The friends of science, must lament that the progress of knowledge is too frequently interrupted by a spirit of national jealousy.

### Note L.

Mr. Davy conceives, that after a complete respiration of atmospheric air, the gas contained in his bronchial vessels, when reduced to the temperature of  $55^{\circ}$ , would amount only to 32 cubic inches, consisting of azote 23, oxygene 4.9, and carbonic acid 4.1 cubic inches. (a) He respired 100 cubic inches of the gaseous oxide of azote for about half a minute, by this process he found, that the gas was diminished to 62 cubic inches, which upon examination, he found were composed of the nitrous oxide 29, azote 25.7, oxygene 4.1, and carbonic acid 3.2 cubic inches. He concludes that 32 cubic inches of gas, of the same composition, will be still left in the lungs; these will consist of nitrous oxide 14.7, azote 13.3, oxygene 2.1, and carbonic acid 1.9 inches. Before the experiment, the gas in the lungs, and in the apparatus, added together

(a) *Researches*, p. 409.

together, were 132 cubic inches, composed of the oxide 100, azote 23, oxygene 4.9, and carbonic acid 4.1 inches; after the experiment, the gases in the lungs, and the apparatus taken together, amounted only to 94 inches, composed of the nitrous oxide 43.7, azote 39, oxygene 6.1, and carbonic acid 5.2 inches. From these data Mr. Davy inferred that 56.3 cubic inches of the oxide has been absorbed, and 16 inches of azote produced. (a) Upon this experiment it may be remarked, that the 32 cubic inches supposed to be in the lungs at the commencement, is probably much less than the actual quantity which they contained. It is not expressly stated by Mr. Davy, that he made a complete expiration before the experiment, but granting this to be the case, we may conjecture, that above 100 cubic inches would be still left in the lungs; now supposing the whole of this quantity to be of the same composition with the part examined by Mr. Davy, it will be composed of about 72 parts azote, 15 oxygene, and 13 carbonic acid; if to these we add 100 cubic inches of nitrous oxide, we shall have the whole amount of gases before the experiment. After the experiment, supposing that a complete expiration was made, there will still be 100 inches left in the lungs; supposing with Mr. Davy, that the residual

(a) *Researches*, p. 412 & seq.

residual gas in the lungs, is of the same composition with that in the apparatus, these 100 inches will be composed of about 46 inches nitrous oxide, 41·5 azote, 6·6 oxygene, and 5·9 carbonic acid. If we add these quantities to the gases left in the apparatus, the whole amount of the residue after the experiment, will be nitrous oxide 75 inches, azote 67·2, oxygene 10·7 and carbonic acid 9·1 inches. These numbers compared with the respective quantities at the commencement of the experiment, will give a deficiency in all the gases to the following amount; nitrous oxide 25 inches, azote 4·8 inches, oxygene 4·3, and carbonic acid 3·9 cubic inches. In making this calculation, it is assumed, that the 100 inches which are supposed to be in the lungs before and after the experiment, are of the same composition with the gas in the apparatus, but of this we have no direct proof. It is impossible therefore, to form any positive conclusions from an estimate founded upon such uncertain data, but the result is unfavourable to the supposition, that azote is evolved in these circumstances; perhaps if the experiment were repeated with the requisite precautions, it would be found, that the only change which is effected, is an absorption of part of the nitrous oxide.

Before



Before the experiment, the gases were,

	Oxygene	Azote	Carb. Acid	Nitr. ox.	Total
In the lungs	15	72	13		100
In the appar.				100	100
Total	15	72	13	100	200

After the experiment,

In the lungs	6.6	41.5	5.9	46	100
In the appar.	4.1	25.7	3.2	29	62
Total	10.7	67.2	9.1	75	162
lost in the ex.	4.3	4.8	3.9	25	38

### Note LI.

The facility with which the greatest part of the oxygene of the atmosphere is absorbed in the various Eudiometrical processes, and the difficulty of separating the last portions of it, so as to obtain the azote in a perfect state of purity, affords a strong presumption, that the component parts of the atmosphere are united together in a state of chemical composition, and not mechanically mixed, or merely diffused through each other's substance. It appears, that in this case, the atmosphere is subjected to the same laws of affinity, which the  
learned

learned M. Berthollet has shewn to exist in all chemical compounds. (a)

### Note LII.

As the red particles can only be rendered visible by the aid of the microscope, the observations that have been made upon them, are deeply tinged with that uncertainty which always attends the use of high magnifying powers. In some instances we are able to point out, upon optical principles, the probable cause of the deception ; but, in other cases, it must be confessed, that there is reason to suspect, that the observer saw what best coincided with his previous hypothesis. A good account of the observations which have been made upon this subject, accompanied with some original experiments, may be found in the appendix to Mr. Cavallo's treatise on the factitious airs.

### Note LIII.

In the Phil. Transf. for 1797, there is a paper on the colour of the blood, written by Dr. Wells, in which the author endeavours to overthrow the generally received opinions upon this subject. He conceives, that the alteration induced upon the

(a) *Ann. de Chim.* t. xxxvi.

the colour of the blood, as well by the action of oxygene, as of some of the neutral salts, is altogether independent of any change effected by them upon its colouring matter. In order to prove this proposition, he made a solution of the red particles (i. e. the colouring matter) in water, and he found, that the colour of this solution was not affected by the same reagents, which produce a change of colour in the crassamentum of the blood. He also found that an aqueous solution of the red particles, whether taken from the crassamentum of the arterial, or the venous blood, exhibited precisely the same shade of colour. Hence he concludes, that the action of air, nitre, and other substances which redden the blood, is exercised not upon the red particles themselves, but upon the medium which surrounds them. This action consists in rendering the serum more dense, and thus causing it to reflect a greater number of rays of light from its internal surface; and he attempted to prove by experiment, that the blood is in fact rendered more dense, in those cases where its colour is brightened. The reasoning of Dr. Wells is certainly ingenious, but, I think, not conclusive. We know, that by solution in water, the peculiar texture and organization of the red particles is destroyed, so that though no alteration of colour be produced by the air upon these bodies,

dies,

dies, when in this state, it does not follow that the change would not be produced, if the air were applied to them while they retained their globular form. Our knowledge respecting the arrangement of the minute particles of bodies, is perhaps too imperfect for us to determine with certainty, by what means the changes of colour are effected, which follow from the application of different chemical reagents; an alteration in the density of the body, may probably be one among other methods; but supposing this to take place in the case now under consideration, it is more natural to conceive, that the action should be exercised upon the particles themselves, than upon the substance which is contiguous to them.

Dr. Wells, in the 2d part of his paper, offers some experiments to shew, that the red colour of the blood does not depend upon the iron contained in the red particles. They certainly deserve consideration, but if we admit them in their full extent, they would prove, that no part of the blood contains iron in the saline state, a position which is directly opposed by the experiments that have been made more lately by Fourcroy and Vauquelin, who have not only confirmed the existence of the iron, but have ascertained, apparently with great exactness, the state of combination in which it exists.

exists. The hypothesis with which Dr. Wells concludes his paper, viz. that the red globules are composed of two distinct substances, possessed of different chemical properties, even though it coincides with the microscopical observations of Mr. Hewson, I cannot but consider as altogether fanciful. Before we attempt to form any more conjectures on this subject, it would be desirable to repeat and vary the experiments of Fourcroy and Vauquelin.

The imagination of the poet has in some degree anticipated the discovery of the chemists.

When air's pure essence joins the vital flood,  
And with *phosphoric acid* dyes the blood,  
Your virgin trains the transient heat dispart,  
And lead the soft combustion to the heart. &c.(a)

Dr. Darwin observes in a note, “ Dr. Crawford, in his ingenious work on animal heat, has endeavoured to prove, that during the combination of the pure part of the atmosphere with the phlogistic part of the blood, much of the matter of heat is given out from the air; and that this is the great and perpetual source of the heat of animals; to which we may add, that

“ the

(a) *Economy of Vegetation* canto i. l. 399—402.



“ the phosphoric acid is probably produced by this  
 “ combination; by which acid the colour of the  
 “ blood is changed in the lungs from a deep  
 “ crimson to a bright scarlet.”

#### Note LIV.

M. Abildgaard, of Copenhagen, made some experiments, of which a sketch is given in the *Annales de Chimie*, (a) in order to determine the comparative quantity of carbone in the arterial and the venous blood. 100 Parts of the venous blood of a horse, when dried by a moderate heat, afforded 26 parts of a dry pulverizable substance, whereas 100 parts of the arterial blood treated in the same manner, yielded only 25 parts. An ounce of venous blood, dried and decomposed in a close vessel, produced  $115\frac{1}{2}$  grs. of carbonaceous matter; the same quantity of arterial blood produced no more than  $87\frac{1}{2}$  grs. These experiments certainly appear favourable to the modern theory of respiration, but the account which is given of them is too concise to enable us to form any decisive conclusions.

#### Note LV.

Dr. Bancroft first suggested the opinion, that  
 charcoal

(a) T. xxxvi. p. 91.

charcoal was an oxide of carbone, (*a*) an opinion which has been since amply confirmed by the experiments of Guyton and Tennant. It is probable that this black oxide of carbone can only be formed by a degree of heat sufficient to produce ignition, and consequently much superior to that of the blood. It may also be presumed, from the whiteness of the chyle, that the carbone which it contains, is not combined with oxygene, or at least, not in that state of oxidation, which constitutes charcoal. Hence we may infer, that the inflammable matter of the blood exists in the state of pure carbone, and consequently, that the carbonic acid which is emitted from the lungs, is formed by the union of 18 parts of carbone, with 82 parts of oxygene. Dr. Bree supposes that the dark colour which the mucus of the bronchial glands sometimes assumes, is owing to a portion of the carbone of the blood, which, in asthmatic patients, is not carried off, in sufficient quantity, by the formation of carbonic acid. (*b*)

The opinion of Mr. Abernethy, respecting the origin of the carbonic acid discharged from the lungs, is very different from that generally assigned by the modern physiologists. He observes,  
 Q *that*

(*a*) *On the Phil. of Colours*, p. 48. note.  
*Respiration*, p. 125.

(*b*) *On*

that “ at one time, physiologists believed that the  
“ inspired oxygenous gas, contributed to the pro-  
“ duction of the carbonic gas, found in the air  
“ expired. This opinion perhaps still prevails in  
“ the minds of some people.” (a) I conceive, how-  
ever, that the doctrine which is here reprobated,  
is at present almost universally adopted. Mr.  
Abernethy supposes that the carbonic acid emitted  
from the lungs, is not properly the product of re-  
spiration ; but is simply exhaled from the pulmo-  
nary vessels. His chief objection to the contrary  
doctrine is, that the quantity of oxygen received  
into the blood by the lungs, is not sufficient to ge-  
nerate the carbonic acid which is expelled. But  
on this point, Mr. Abernethy’s opinion is in di-  
rect opposition to that of M. Lavoisier, and the  
most eminent chemists, who conclude from their  
experiments, that the quantity of oxygen ab-  
sorbed, is considerably more than sufficient to  
form the carbonic acid.

The opinion of M. Burdin, concerning the for-  
mation of the carbonic acid emitted from the lungs,  
is in some respects similar to that embraced by  
Mr. Abernethy, and is in like manner very dif-  
ferent from the one generally adopted. He  
imagines that all the ingredients of the acid exist  
in

(a) *Essays*, p. 146.

in the venous blood, previous to its return to the lungs, and that when it arrives at this organ, they are united, and separated from it by secretion. He supposes that oxygene is at the same time absorbed by the blood, but that it has no immediate share in the formation of the carbonic acid. (a) In opposition to this hypothesis it may be urged, that the absorption of oxygene and the production of carbonic acid, take place in the same manner, when a piece of crassamentum is placed in a jar filled with oxygene, as when the venous blood is acted upon by the air contained in the vesicles. From this circumstance it follows, that the effect produced depends altogether upon a chemical affinity between the oxygene, and one or more of the constituents of the blood, and not upon the operation of a function which can only subsist during life.

### Note LVI.

In forming this conclusion, I decide not only in opposition to the opinion of M. Lavoisier, but of nearly all the physiologists who have treated upon this subject since the publication of his memoirs. The opinions of the French chemists, as far as I am acquainted with their works, almost

Q 2 uniformly

(a) *Course of Med. Studies*, v. iii. p. 343.

uniformly coincide with that of their illustrious countryman. (a)

Among the Germans I find the same doctrine supported by Blumenbach, Jacquin, and Gren, (b) and in our own country by Crawford, Cavallo, Higgins, Currie, Thomson, Rutherford and Allen. (c)

It is to be observed, that the formation of water is equally owing to respiration, whether in conformity with the opinion of M. Lavoisier we suppose it to be immediately generated in the lungs, or in passing along the sanguiferous system, according to Hassenfratz's hypothesis. In both cases it is produced by the union of oxygene absorbed

(a) *Guyton-Morveau, Enc. Meth. Chimie, t. i. p. 727. 765. Seguin, Ann. de Chim. t. xxi. p. 225. & Med. Eclairée. Fourcroy, Systeme, &c. t. x. p. 373. t. i. p. 56. Hassenfratz, Ann. de Chim. t. ix. p. 261. Vauquelin, Ann. de Chim. t. xii. p. 290. De la Metherie, Journ. de Phys. t. xlv. p. 106. Cuvier, Leçons d'Anat. comp. t. i. p. 92. Richerand, Phys. p. 149.*

(b) *Blumenbach, Inst. Phys. p. 115. Jacquin, El. of Chemistry, § 956. Gren, Ann. de Chim. xxiv. p. 196.*

(c) *Crawford on Animal Heat, p. 154. Cavallo's Elem. of Nat. Phil. v. ii. p. 540, 1. Higgins's Minutes of a Society, p. 163. Currie's Med. Reports, p. 192. Thomson's Chemistry, v. iv. p. 497. Rutherford in Marcet de Diabete, p. 31. Allen in De la Rive de Cal. Anim. p. 35.*



forbed by the blood in the lungs, and hydrogene previously existing in that fluid.

Note LVII.

M. Lavoifier, in the two memoirs on respiration, written in conjunction with his friend Seguin, to which I have already so frequently referred, employs no arguments to prove the discharge of hydrogene from the blood in addition to those stated above: as however these papers contain an account of the most elaborate experiments that have ever been performed upon the subject of respiration, and are the last works which this great philosopher lived to complete, it may be desirable to examine them with some degree of minuteness.

After stating some general propositions respecting the nature of combustion, and the composition of the bodies which are produced by respiration, they proceed to observe, that this function may be considered as a slow combustion of carbone and hydrogene, and that its effect is to abstract from the blood a quantity of these substances, and to deposit there a portion of caloric. There is, however, one circumstance, in which respiration is materially different from combustion;

combustion ; this latter operation proceeds with more rapidity in proportion to the greater purity of the air employed, whereas the quantity of oxygene which an animal consumes in respiration, is very nearly the same in whatever proportion it exists in the air received into the lungs. This proposition was proved by a series of experiments performed upon guinea-pigs. These animals were confined for some days in air of different composition, from pure oxygene on the one hand, to air which only contained  $\frac{1}{13}$  part of this substance. We are expressly told, that under these circumstances, the respiration and circulation were not affected, and that the heat continued the same ; in short the only perceptible alteration in the state of the animal was a degree of drowsiness, when too large a proportion of azote had been employed. There appears no reason to doubt the accuracy of these experiments, the conclusions from them are probably different from what we should have been led to expect ; they however demonstrate the absolute inefficiency of the attempts which have been of late made with so much confidence to introduce at pleasure into the system, a greater or less quantity of oxygene, merely by varying the composition of the air presented to the lungs. The authors state, that during respiration there is neither any disengagement

gagement nor absorption of azote; they consider this substance as entirely passive, and assert that any other gas mixed with the oxygene, which was not immediately noxious, would answer the same purposes with the air of the atmosphere; they employed hydrogen, and found no inconvenience to arise from its use. It had been observed, that the quantity of oxygene consumed in a given time, was different in different states of the animal œconomy; the authors proceed to investigate the amount of these changes, and for this purpose M. Seguin himself became the subject of the experiments. The circumstances which they noticed as affecting the process of respiration were, temperature, exercise, and the state of digestion. As a standard for the rest of the experiments, they began by taking an average effect produced by a man at rest, with the stomach empty, and at the temperature of  $82^{\circ}$ ; under these circumstances he consumed in the space of an hour 1210 cubic inches of oxygen. At the temperature of  $57^{\circ}$ , the other circumstances remaining as before, he consumed 1344 inches. During the process of digestion, the consumption of oxygene was increased to 18 or 1900 inches. While the stomach was empty, and the body in a state of violent exercise, 3200 inches were consumed in an hour, and when he

used

used the same exertion after taking food, the quantity was increased to 4600 cubic inches in the same space of time. Notwithstanding this great difference in the quantity of oxygene consumed, the temperature of the body was scarcely affected, but the circulation, and the respiration, were considerably quickened. The authors observe, that these numbers must only be considered as indicating proportions, because it is probable, that individuals differ in the absolute quantity of the changes which are effected by the respiration. The apparatus by which these experiments were performed, is not specified. It must be observed that the effects produced upon the respiration by a change of temperature, had several years before been distinctly pointed out by Dr. Crawford, and M. Jurine had also ascertained that muscular exertion, and the state of the stomach, produced very important effects upon this function, yet neither of these authors are noticed in the memoir now under consideration.

Taking into account all these different circumstances, the authors conclude, that about 1 cubic foot, or 1728 inches, may be considered as the average quantity of oxygene consumed in an hour, and consequently 24 cubic feet in the course of a day. They next proceed to point out

out what proportions of this oxygene are to be respectively assigned to the production of the carbonic acid, and the water. It is not expressly stated, in what manner the sum of the carbonic acid was obtained, but this we may suppose to be sufficiently accurate. It is then assumed, that this carbonic acid is composed of carbone, which previously existed in the blood, in the same state of oxidation with charcoal, and the additional quantity of oxygene is estimated, which is required to form it into complete carbonic acid. The remainder of the oxygene is supposed to be employed in the production of water, and by calculating how much water can be formed by this superabundant oxygene, the amount of the aqueous exhalation from the lungs is estimated. It will not be necessary to follow the authors through their details; I have endeavoured to shew that their calculations are built upon the assumption of data, one of which is improbable, and the other at least incapable of proof. The memoir concludes with some general reflections deduced from the experiments. With respect to the increased consumption of oxygene in a low temperature, the authors are inclined to attribute it to the greater density which cold air possesses, and to suppose, that the principal circumstance which moderates the heat of the body in a high temperature,



temperature, is the increase of transpiration, which carries off part of the caloric that had been previously evolved, by the formation of the water and carbonic acid.

Before I leave this memoir, I must remark, that in the estimate which is made of the quantity of water formed in the lungs, the figures which denote the weight of the hydrogenic discharged, and of the water produced, do not correspond. The whole quantity of oxygen consumed is 19080 grs.; the carbonic acid produced is 21600 grs.; this will contain 15553 grs. of oxygen. There will then be left for the formation of water 3528 grs., which will require 622 grs., or 1 oz. 46 grs. of hydrogen, and the quantity of water produced will be 4150 grs., or 7 oz. 1 dr. 46 grs. In the memoir under consideration, the quantity of hydrogen is stated to be 1 oz. 5 drs. 51 grs. and the water only 5 drs. 51 grs.

The 2d memoir written by M M. Lavoisier and Seguin, is expressly on the transpiration of animals. Transpiration is described as consisting in the loss of a certain moisture, which requires a quantity of heat to reduce it to vapour, in order to carry it off from the body; it proceeds both  
from

from the skin and the lungs, and according as it is derived from the one or the other of these sources, it is stiled the cutaneous, or the pulmonary transpiration. Upon the greater or less quantity of this transpiration, together with the degree of density in the inspired air, the authors conceive, that the uniformity of temperature depends, which the animal preserves in all the different degrees of external heat to which it is exposed. In addition to the aqueous vapour derived from the cutaneous and pulmonary transpiration, there is also the water supposed to be generated in the lungs from the immediate union of oxygene and hydrogene; it remained therefore to contrive a method by which the effect of these operations might be kept distinct and precisely ascertained; for this purpose the following apparatus was employed. A person was enclosed in a silk garment covered with a varnish of elastic gum, he had a tube adapted to his mouth, and by this method it is stated, that the effects of transpiration and of respiration were kept distinct. But in this statement there appears to be an inaccuracy; the effects of cutaneous transpiration would certainly be kept distinct from the effects of pulmonary transpiration and respiration, but it does not appear how these two were to be separated.

By

By weighing the body just after entering the apparatus and just before leaving it, they endeavoured to ascertain the loss of weight which the body experiences by respiration, separate from that by transpiration. By weighing the body immediately after entering the apparatus, and immediately after leaving it, the total loss which it experiences by all the different sources is ascertained. The authors conjecture, that there exudes into the bronchia a humour which is separated from the blood, composed chiefly of hydrogene and carbone, and which filters through the membranes of the lungs. This humour, by being very much attenuated as it passes through the small exhalants, is in part consumed, and by decomposing the oxygene with which it is in contact during inspiration, water and carbonic acid gas are produced. The authors observe, that the carbonic acid is removed from the lungs by expiration, but that the water would be accumulated, were there not a method provided by nature to prevent this circumstance. The air enters the lungs cold, but issues from them warmed nearly to the temperature of the blood, it will, therefore, dissolve a part of the water, and remove it from the lungs. From this hypothesis, which supposes that the combustible matter, disengaged during respiration, is, in the first instance, trans-

mitted

mitted through the pores of the lungs, in the form of an aqueous exhalation, it might be supposed quite superfluous to assign any other source for the water of expiration ; but it is immediately added, that the water exhaled is derived from two sources, 1st. the water of pulmonary transpiration which we have been describing, and 2d. that produced by the immediate union of oxygene and hydrogen, or what is styled the water of respiration. We are informed, that in a future memoir the method will be detailed which was employed to separate the water derived from these two sources, and to ascertain their respective amounts ; but this memoir appears never to have been published, in consequence, it may be presumed, of the untimely fate of M. Lavoisier.

The authors proceed to inform us, that by ascertaining what quantity of water was sent out of the lungs, and what quantity of carbonic acid was produced, and afterwards, by comparing these quantities with the quantity of oxygene consumed, an estimate was formed of the water generated in the lungs, and of that which was evaporated from them. From the expression employed there is reason to infer that the water exhaled was by some method, which is not mentioned in the memoir, actually collected and measured,

fured, and not as in the former instance simply calculated by a comparison with the oxygene consumed. But, supposing this to be accurately performed, before the conclusions could be obtained which are formed by Lavoisier and Seguin, it would be necessary to prove that all the oxygene was employed in the formation of carbonic acid and water, and that the carbone had previously existed in the state of charcoal. They seem to have been aware of the difficulty respecting the carbonic acid, and add, that if the carbonic acid be produced by digestion, the oxygene must be employed in the formation of the water of respiration, which will diminish the quantity supposed to be derived from the pulmonary transpiration; but it is still taken for granted, that all the oxygene is employed in uniting with the combustible matter discharged from the blood. The mean results of the experiments are then detailed, and are as follows:—the loss of weight experienced by the body in twenty-four hours is 2lb. 13oz.; this includes the loss by respiration, and by both the cutaneous and the pulmonary transpiration; of this quantity the cutaneous transpiration takes away 1lb. 14oz., and the respiration 15oz.; it appears by a subsequent part of the calculation, that this estimate of the loss by respiration includes also the loss by the pulmonary transpiration;



transpiration ; it appears, indeed, that these two effects were necessarily confounded from the nature of the apparatus. The mean consumption of oxygene in twenty-four hours is 2lb. 1 oz. 1dr. 10gr. ; this oxygene is employed partly in the formation of carbonic acid, and partly in the formation of water. The weight of carbonic acid disengaged in twenty-four hours is 1lb. 1 oz. 7dr. 4gr. consisting of oxygene . . . 12oz. 0dr. 4gr. carbone . . . 5oz. 7dr. 0gr.

1lb. 1 oz. 7dr. 4gr.

There is then left a quantity of oxygene equal to 1lb. 4oz. 2dr. 10gr. which, in order to be formed into water, would require 3 oz. 3dr. 10grs. of hydrogene, which will produce a quantity of water equal to 1lb. 7oz. 5drs. 20grs.

The 2lbs. 13oz. which are supposed to be lost in twenty-four hours, will be found then to consist of the following quantities: *lb. oz. dr. gr.*

Cutaneous transpiration	oz.	dr.	gr.	1	14	0	0
-------------------------	-----	-----	-----	---	----	---	---

Carbone in the carb. acid 5 7 0

Hydrogene in the water 3 3 10

Pulmonary transpiration	5	5	62
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Loss by respiration . . . . .	0 15	0 0
-------------------------------	------	-----

2 13 0 0

In

In this calculation it may be observed, that the two first quantities only are the direct result of experiment. The hydrogen was estimated from the oxygen left after the formation of the carbonic acid gas, and the pulmonary transpiration was a quantity assumed to supply the difference between the weight of the hydrogen and the carbon, and the total amount of the loss by respiration. It has been already noticed, that the different experiments of Lavoisier himself, indicate very different quantities of oxygen consumed, and of carbonic acid produced in the process of respiration.

Upon the whole, we may conclude, that these memoirs still leave us much in doubt respecting the nature of the changes produced upon the blood by respiration, still less can they be considered as ascertaining with exactness the amount of these changes. At the same time we must acknowledge, that they exhibit an ingenuity in the contrivance, and a perseverance in the execution of experiments which so justly entitle their illustrious author to the first rank among chemical philosophers.

As my object in the analysis of these papers was, rather to present a view of the method of reasoning employed by Lavoisier, than to enter into

into an examination of his results, I have retained the figures used in the original, which indicate the weights and measures of France before the revolution.

## Note LVIII.

I have reserved the discussion of the subject of animal heat to a subsequent part of the essay; I shall, however, in this place remark, that the doctrine which I have attempted to establish concerning the non-emission of hydrogen by the lungs, will materially affect our opinions respecting the influence of respiration upon the temperature of the living body. According to the generally received theory, the whole, or a part of the water which is discharged from the lungs, having been formed in the system by the union of the gasses which form its component parts, produces the extrication of caloric in the same manner as during the generation of the carbonic acid gas. But if, on the other hand, we suppose that the water is generally received into the system by the stomach, in the fluid state, and is afterwards emitted from the body in the form of vapour, its discharge will be necessarily attended with a considerable expenditure of caloric. This removal of caloric from the body will take place in exact proportion to the quantity of water evaporated, either from the

skin or the lungs, and consequently we must consider the discharge of aqueous vapour, either by the pulmonary or the cutaneous transpiration, as in all instances counteracting or moderating the effects produced by the discharge of carbone from the blood. According to this view of the function of respiration, the process by which the body is cooled in high temperatures, is not confined to the operations of the skin, but is effected in an equal or probably greater degree by the lungs, so that we must regard this organ as possessing the power of imparting or abstracting heat to and from the system, according to the temperature of the surrounding medium, or the occasional demands of the animal. The application of this hypothesis to the operations of the living body, as well in the state of health as of disease, is obviously of great extent and importance, but I shall not enter into any speculations concerning it until I have endeavoured, by experiment, to establish its validity. I shall only remark, that I think it will materially assist in the elucidation of those anomalous facts in animal heat, which some eminent modern physiologists have conceived to be inexplicable upon the theory of Dr. Crawford. I allude particularly to the experiments and observations of the late Mr. Hunter, and to those more recently made by my much respected friend, Dr. Currie.

Note

## Note LIX.

An hypothesis similar to this of M. Cuvier's, has been more lately proposed by Dr. Thomson; (a) it is advanced by him, as an original suggestion, and from the candor which pervades his writings, we must conclude, that he had not seen the work of M. Cuvier when he wrote the article of Animal Substances, in the *Supp.* to the *Enc. Brit.* The *Leçons d'Anatomie Comparée*, were published in 1800 (ann. 8), Dr. Thomson's essay in 1801. M. Cuvier and Dr. Thomson, in conformity with the prevailing opinion, conceive that both carbone and hydrogen are removed from the chyle when it is converted into blood, but we have no proof that the proportion of hydrogen is greater in chyle than in fibrine; the experiments that have been performed upon this subject only prove, that azote exists in greater proportion in blood than in chyle.

The hypothesis which is adopted by M. Richerand, to account for the more complete animalization of the chyle, appears singularly perplexed. He supposes, that, as it passes through the lungs, after being mixed with the venous

R 2

blood,

(a) *Chemistry*, v. iv. p. 497.



blood, a great part of it is deposited, “ par une  
 “ forte de perspiration interieure,” into the pa-  
 renchymatous substance of these viscera. In this  
 situation it is oxidated by the contact of the air,  
 and being re-absorbed by a number of inhalent  
 vessels, it is carried to the bronchial glands, where  
 it deposits part of its carbone, “ et de fuligineux,”  
 and thus communicates to them their black colour.  
 Being thus purified, it is returned into the tho-  
 racic duct, and poured again into the subclavian  
 vein, whence it is brought back to the lungs,  
 and again subjected to the action of the air.  
 “ De maniere qu’il se fait,” says the author,  
 “ à travers ces organes, une veritable circulation  
 “ lymphatique, dont l’object est de donner au  
 “ chyle un degré d’animalisation plus avancée.”(a)

#### Note LX.

In ordinary experiments, oxygenous gas is  
 obtained from a metallic oxide, heated in con-  
 tact with the sulphuric acid. But these oxides  
 not unfrequently contain a quantity of carbonic  
 acid, which will necessarily be expelled by this  
 process, and mixed with the oxygene; this is  
 particularly the case with the red oxide of lead,  
 a substance which frequently is made use of for  
 the

the disengagement of vital air. The black oxide of manganese, which is often employed for the same purpose, is not unfrequently mixed with the carbonate of lime, which will give out its carbonic acid during the process. (a) When the heat has been applied for a considerable length of time, and been in any degree violent, part of the sulphuric acid itself will be converted into vapour, and be mixed with the gas. It is to accidental impregnations of this nature, that we must attribute the painful sensation which some persons have experienced in attempting to respire oxygen.

### Note LXI.

Some persons have inferred from Dr. Priestley's experiments, that the respiration of oxygen is productive of injurious effects to the animal constitution. (b) This opinion was, I imagine, founded upon the observation of this philosopher, that after an animal had expired in oxygenous gas, a 2d could continue to live for a short time in the same air. Dr. Priestley, however, attributed the death of the first animal to the effect of cold, in consequence

(a) *Cavallo on Fact. Airs*, p. 203.  
*Fact. Air*, part i. p. 13.

(b) *Beddoes on*

sequence of its passing through the water by which the gas was confined, and he accordingly was able to prolong the life of the animal for several hours, until the air was completely deoxidated, by placing it near the fire. "*This experiment,*" he adds, "*fully satisfied me, that it was nothing in the dephlogisticated air itself, that was the reason that mice could not live in it.*" (a) The deficiency of external heat appears evidently to have had an important effect upon the life of the animal, but there is another circumstance, which I am inclined to regard as a still more immediate cause of death, viz. a quantity of carbonic acid gas, which would be generated, and be confined in the apparatus. A fresh and vigorous animal would be able for some time to bear with impunity a quantity of this gas, which had proved fatal to one whose powers were exhausted by the long continued exposure to it. In confirmation of this opinion, I may remark, that the very same circumstance has been found by Cigna, Jurine, Morozzo, and others, to occur in the respiration of a limited quantity of atmospherical air. (b)

M. Lavoisier

(a) *Exper. on Air*, v. ii. p. 165. (b) *Journal de Phys.* t. xxv. p. 105, 6. *Chaptal's Chemistry*, v. i. p. 131, 2. *Enc. Meth. Art. Med.* t. i. p. 496.

M. Lavoisier, in some of his earlier experiments made the same observation, viz. that after the death of the first animal, the air was left in sufficient purity, to support for some time the life of a second. The same explanation may be had recourse to in these, as in the experiments of Dr. Priestley; indeed, as M. Lavoisier confined his gas by mercury, there would be a greater proportion of carbonic acid mixed with the oxygene, than when water was employed, as this fluid would necessarily absorb part of the acid. When animals have been confined in oxygene, and the carbonic acid has been carefully removed by a pure alkali, or by lime water, the animal has completely absorbed the whole of the oxygene without any inconvenience. (b)

We may remark, that the experiments of Dr. Priestley, in which he found an animal confined in oxygenous gas, to suffer so much from the effects of cold, afford a strong presumptive argument against the increase of temperature, which some speculative chemists have imagined, must be produced by the respiration of oxygene.

The

(a) *Higgins's Minutes of a Society*, p. 132.

The only other observations upon this subject, which I find in the writings of Dr. Priestley, are contained in the following paragraph.

“ From the greater strength and vivacity of the  
 “ flame of a candle, in this pure air, it may be  
 “ conjectured, that it might be peculiarly salutary  
 “ to the lungs in certain morbid cases, when the  
 “ common air would not be sufficient to carry  
 “ off the phlogistic putrid effluvium fast enough.  
 “ But, perhaps, we may also infer from these  
 “ experiments, that though pure dephlogisticated  
 “ air might be very useful as a *medicine*, it  
 “ might not be so proper for us in the usual  
 “ healthy state of the body; for as a candle  
 “ burns out much faster in dephlogisticated than  
 “ in common air, so we might, as may be said,  
 “ *live out too fast*, and the animal powers be  
 “ too soon exhausted in this pure kind of air.” (a)

It can scarcely be conceived, that Dr. Beddoes has built his positive assertion on the mere speculation of Dr. Priestley; a speculation ingenious indeed, and, at the time when it was formed, plausible, but which has been since controverted by the direct experiments of M. Lavoisier.

#### Note LXII.

“ On fait que la combustion, toutes choses  
 égales

(a) *Exper. on Air*, v. ii. p. 168.



“ égales d'ailleurs, est d'autant plus rapide, que  
“ l'air dans lequel s'opère, est plus pur. Ainsi, par  
“ exemple, il se consomme dans un temps donné  
“ beaucoup plus de charbon ou de tout autre com-  
“ bustible, dans l'air vital, que dans l'air de l'atmo-  
“ sphère. On avoit toujours pensé, qu'il en étoit  
“ de même de la respiration ; qu'il devoit s'accé-  
“ lérer dans l'air vital, and qu' alors il devoit se dé-  
“ gager soit dans le poumon, soit dans le cours de  
“ la circulation, une plus grande quantité de calo-  
“ rique. *Mais l'expérience a détruit toutes ces opi-  
“ nions qui n'étoient fondées que sur l'analogie.*  
“ Soit que les animaux respirent dans l'air vital pur,  
“ soit qu'ils respirent ce même air, mélangé avec  
“ une proportion plus ou moins considérable de gaz  
“ azote, la quantité d'air vital qu'ils consomment,  
“ est toujours la même, à de très-légères différences  
“ près. Il nous est arrivé plusieurs fois, de tenir  
“ un cochon d'Inde pendant plusieurs jours, soit  
“ dans l'air vital pur, soit dans un mélange de  
“ quinze parties, de gaz azote & d'une d'air vital,  
“ en entretenant constamment les mêmes propor-  
“ tions ; l'animal dans les deux cas est demeuré  
“ dans son état naturel ; sa respiration & sa circu-  
“ lation ne paroissoient pas sensiblement, ni acce-  
“ lérées, ni retardées ; sa chaleur étoit égale, &  
“ il avoit seulement, lorsque la proportion de gaz  
azote

“ azote devenoit trop forte, un peu plus de disposition à l’affoupissement.” (a)

### Note LXIII.

Soon after it was discovered that oxygenous gas possesses the appropriate power of supporting animal life, the idea was suggested of employing it for medicinal purposes. As it was known, that in phthisis the lungs are much injured in their texture, and probably rendered incapable of performing their functions to the full extent, some of the French chemists conceived that the respiration of oxygene might prove advantageous in this disease, by permitting the system to acquire its due proportion of this element with greater facility. The idea was at least as plausible as the generality of medical hypotheses, and the subjects of the first experiments, in consequence of the happy delusion which so frequently attends this fatal complaint, were inspired with strong hopes of the success of the new practice. (b)

Shortly after this period a speculation of an opposite

(a) Lavoisier, *Mem. Acad.* 1789. p. 573. (b) *Chaptal's Chemistry*, v. i. p. 138. *Jurine, Enc. Meth. art. Médecine*, t. i. p. 500. *Fourcroy, Ann. de Chim.* t. iv. p. 83. & seq.

opposite nature was formed in this country. It was imagined that the system, when affected by phthisis, had in some way acquired too large a proportion of oxygene in its composition, and it was consequently concluded that benefit must be derived from the respiration of a gas which contained a smaller proportion of oxygene than the air of the atmosphere. The objections which were urged against this hypothesis were indeed numerous, and the defenders of it were unable to point out by what means the system could acquire this superabundant quantity of oxygene. But all opposition was for some time silenced by the apparent success which attended the employment of the pneumatic medicines, and so fully persuaded were the profession at large of the advantage likely to result from their use, that a public subscription was actually commenced, in order to form an institution, for the purpose of curing consumption by the employment of the different gases. But the infatuation has at length subsided, and the plan seems to be abandoned by the projector himself, who cannot be supposed to want either zeal or industry in the promotion of his favorite scheme.

#### Note LXIV.

Few persons, I conceive, in the present day,  
will

will object to this expression. The pertinacity with which the opposite opinion was maintained by the determined followers of the Brunonian doctrine, appears to me one of the most singular contests of theory against experience, which has occurred in the history of modern science. Dr. Smith, in his valuable dissertation "*de actione musculari*," ranks "*vapores mephitici*" among the chemical sedatives. (*a*)

(*a*) *p.* 33.

END OF THE SECOND PART.

*In collecting the materials for this work, I have frequently found great difficulty in obtaining the necessary information, in consequence of the imperfection or inaccuracy of references. In order to prevent the recurrence of this obstacle to any future inquirer into the subject of respiration, I have noted my references with as much accuracy as lay in my power; and to remove the difficulty which is sometimes produced by the employment of different editions of the same work, I have subjoined a list of the books and treatises to which I have referred, marking the particular editions that were employed.*

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